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THE DEPARTMENT OF ENERGY'S RESTRUCTURED FUSION ENERGY SCIENCES PROGRAM

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The Department of Energy's Restruct...

HEARING

BEFORE THE

SUBCOMMITTEE ON ENERGY AND ENVIRONMENT

OF THE

COMMITTEE ON SCIENCE

U.S. HOUSE OF REPRESENTATIVES

ONE HUNDRED FOURTH CONGRESS

SECOND SESSION

MARCH 7, 1996

[No. 53]

Printed for the use of the Committee on Science

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*Ranking Minority Member
**Vice Chairman

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THE DEPARTMENT OF ENERGY'S RESTRUCTURED FUSION ENERGY SCIENCES PROGRAM

THURSDAY, MARCH 7, 1996

U.S. HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE,
SUBCOMMITTEE ON ENERGY AND ENVIRONMENT,
Washington, DC.

The Subcommittee met at 9:37 a.m. in Room 2318 of the Rayburn House Office Building, the Honorable Dana Rohrabacher, Chairman of the Subcommittee, presiding.

Present: Representatives Rohrabacher, Roemer, Fawell, Bartlett, Wamp, Graham, Baker, Ehlers, Olver, McHale, Johnson, Rivers, McCarthy, Jackson-Lee and Volkmer.

Chairman ROHRABACHER. The Energy and Environment Subcommittee will come to order. Today, we will consider the Department of Energy's Restructured Fusion Energy Sciences Program.

And, Mr. Roemer, are you ready?

Mr. ROEMER. Yes, sir.

Chairman ROHRABACHER. Mr. Brown? All right. Forty years and $14 billion, that's what we are talking about.

Forty years and $14 billion, using constant 1995 dollars, that is the investment this country has made in the quest to develop fusion energy as a practical source of power. A year ago, we were looking at a fusion program outline that would have nearly doubled the annual cost within a time line well into the next century.

That game plan ignored budget realities and set no priorities. Following it would have meant another 40 years and another $14 billion, at least.

I think even its strongest supporters would agree that this unrealistic approach was more harmful than helpful to the development of fusion energy in America.

The fiscal year 1996 Energy and Water Appropriations signed into law by the President wisely called for a restructured program more in line with budget realities. To its credit, the Fusion Energy Advisory Committee, charged with this responsibility by the Department of Energy, has done just that.

In its report, the Committee states, "The underlying theme of restructuring is to redirect the program away from the expensive development path leading to a fusion power plant to focus on less costly, critical, basic science and technology foundations." This is commendable. Yet, the policy recommendations outlined in the report do not seem to follow this direction.

The Committee's proposal would have us spend the bulk of the fusion budget on a variety of projects centered around the tokamak concept. For example, all three tokamak programs would continue in fiscal year 1997, although the TFTR facility at Princeton was originally scheduled for termination last fall.

In a letter to me, dated March 4th, which, without objection, I shall submit for the record—and do we have that here? (The letter referred to appears in Appendix I, on page 157.)

Professor Lawrence Lidsky of MIT, who worked on the Plasma Physics and Fusion Technology Programs for over 20 years now, said the fusion program does not deserve support as long as it continues its "relentless focus"—and I put that in quotes, "relentless focus"—on the development of a tokamak power plant, which Professor Lidsky describes as large, expensive, unforgiving of error and intensely radioactive.

We have also heard that this relentless focus has shoved aside serious consideration of alternatives to the tokamak concept. Our witnesses today, representing a wide spectrum of views, will allow us to determine whether these criticisms are justified.

All are agreed the Fusion Energy Program is at a critical point. I believe this hearing will be helpful in giving the members of this Subcommittee guidance, the guidance they need, to make the budget choices for fiscal year 1997.

First, we will hear from the Chair of the Fusion Energy Advisory Committee and a distinguished group of scientists who have varying views on the direction of the program. Later, we will hear from the Department of Energy, a dissenting member of the Advisory Committee and outside citizen groups for their reaction to the report and policy recommendations.

But, before I introduce the first panel, let me turn to the gentleman from Indiana, our distinguished Ranking Minority Member, Mr. Roemer, for his opening remarks. And, I just might—out of courtesy, Mr. Roemer, perhaps we should extend this courtesy first to former Chairman Brown.

Mr. Brown, do you have any opening remarks? And, then we will have Mr. Roemer sum it up for your side.

Mr. BROWN. Let me merely express my thanks to you for this continuing investigation of the future of the Fusion Energy Program. It's a subject matter that I have been closely related to as long as I've been in the Congress and will continue to be closely related to.

And, I have no further remarks. But, if I do happen to come up with some, I ask unanimous consent to put them in the record.

Chairman ROHRABACHER. Without objection. And, we are again very pleased to have the former Chairman with us to give us not only his expertise but his institutional memory of some of these issues.

Mr. Roemer, would you like to proceed?

Mr. ROEMER. I would. Thank you, Mr. Chairman. And, I would like to again extend my wishes for a very happy birthday to our former Chairman, Mr. Brown, as well, too.

[Laughter.]

Mr. ROEMER. It was a few days ago. No more, though. This is the last birthday for Mr. Brown.

Chairman ROHRABACHER. And, they are developing a candle that just won't blow out here, Mr. Brown.

Mr. ROEMER. Mr. Chairman, I applaud you for convening this hearing to debate and discuss the fusion R&D programs at the Department of Energy. I have supported fusion energy R&D since I came to Congress, and I look forward to authorizing this program for FY-1997.

The question we face today is whether the $120 million or one-third cut to the fusion program last year was too much or too little. I believe that the cut was too much and have advocated restoring a small portion of the lost funds to the program.

In Fiscal Year 1996, Congress allocated $244 million for the fusion program. That went from $366 million—and I reiterate—to $244 million.

As we will hear today, the Fusion Energy Advisory Committee has recommended a funding level of $275 million for FY-1997, a recommendation with which I agree. Even at $275 million, however, the program will have lost over one-fourth of its budget in two years and will remain well below the funding profile that President Clinton originally sought.

In view of the rancorous, often partisan debate over the fusion budget last year, I am heartened by a renewed bipartisan support for the program. Nearly 50 members recently co-signed a letter with myself and Mr. Bartlett asking the Secretary of Energy to provide $275 million for the fusion R&D program in FY-1997.

Among the 50 co-signers were six Republican members of the Science Committee and seven Appropriation Committee members. These members, and many others in Congress, recognize the tremendous potential of fusion, the energy source of the sun and hydrogen bombs, to produce electricity.

Along with solar and renewable energy sources, fusion energy can power American homes and vehicles when the much feared day comes that fossil energy runs out. As we will hear next week, that day may be further away than we originally thought.

However, no American wants the country to give up on developing new, clean energy sources, especially since fossil sources will eventually be gone and since pollution and oil supply disruptions will plague us in the meantime.

For over 40 years, U.S. scientists have been trying to harness the power created in the hydrogen bomb to generate electricity. Flush on the success of the Manhattan project in World War II, scientists claimed that fusion energy would power American homes by 1990.

However, nature proved more illusive than they thought. Where there have been many advances in fusion science, most often based on a fusion concept known as tokamak, cheap and plentiful fusion energy is still decades away from being a reality.

Some people argue that we can, therefore, cancel the quest for fusion power, especially since solar and renewable energy technologies are producing commercial power right now. But, I wonder.

Are Americans unhappy that they can buy CDs because records seem to work just fine? Are Americans mad at makers of microwaves because their kitchens already had ovens?

Americans will need lots of energy for the foreseeable future. So, I believe it is, at best, premature to decide that only certain technologies will work best for Americans in the 21st century.

At the same time that I am enthusiastic for fusion energy R&D, I also have some questions about the direction of the fusion R&D program at DOE. In particular, I wonder whether the United States should fully commit to ITER and the ITER project after the design phase is complete.

There have been proposals for a scaled back commitment. What are the costs of the construction? I also question whether university programs are sufficiently funded within the current budget.

Ultimately, as a fiscal conservative, I am glad that we can save money in the fusion program and still address the need for plentiful, inexpensive power. However, again, as a Democrat who wants a balanced budget, I am mindful that these cuts may forsake the U.S. leadership position in fusion R&D and, thereby, preclude the opportunity for U.S. development of promising fusion technologies.

I hope that through the budget process this year we can strike a balance between the competing goals of fiscal constraint and restraint and of investment in America's competitive future.

I look forward to hearing the testimony of the witnesses that have come forward today to share their views on the fusion budget and on their hopes for the fusion program in the future.

Thank you, Mr. Chairman. And, I look forward to this debate.

Chairman ROHRABACHER. And, Ms. McCarthy, I understand you do not have an opening statement?

Ms. MCCARTHY. No, thank you.

Chairman ROHRABACHER. Thank you very much for being with us today.

Our first panel includes distinguished scientists in the field who have participated in or commented on the Fusion Energy Advisory Committee report. And, just so you gentlemen will know, I generally try to encourage dialogue at my hearings.

I was always dismayed at times in the past when I went to different committees, you would have all the witnesses on one side of the issue, which happened to be the side of the issue the Chairman agreed with, in the first panel. And, then you would have perhaps someone who disagreed, one or two people, late in the afternoon.

And, I, instead, believe that we should have both sides represented on all panels and that we should encourage dialogue, because you are the folks that understand and have the expertise that will permit the level of discussion that will go beyond just these chambers, that will be heard in the universities and the science labs throughout the United States. So, I appreciate you being here.

I encourage that you give—if you can only give your full statement, that's fine. But, if you would like to condense it and go to your central points, it will probably leave more time for a dialogue and a discussion on the issues that you think are important.

First, we have with us Dr. Robert Conn. He is the Walter J. Zable Professor of Engineering at the University of California, San Diego. Dr. Conn chaired the Fusion Energy Advisory Committee.

He has brought with him two people who will not be testifying. Would you like to introduce them?

Dr. CONN. Yes, thank you, Mr. Chairman. We had two Chairs of Subcommittees of our Committee who did a great deal of the work that's in the report.

The first is Dr. Michael Knotek, who is sitting to my left immediately. He is from the Pacific Northwest Laboratory. And, the other is Professor Jim Callen, who is Professor of Nuclear Engineering and Engineering Physics at the University of Wisconsin, Madison.

Chairman ROHRABACHER. So, they are available to us as resources as we get into this discussion.

Mr. CONN. That's our understanding, yes.

Chairman ROHRABACHER. And, I appreciate you both being with us today.

And, then we have with us Professor William Drummond. He comes to us from the Fusion Research Center at the University of Texas at Austin.

And, then Professor George Miley is Director of the Fusion Studies Laboratory at the University of Illinois.

And, Doctor John Perkins, who works with the Magnetic Fusion Energy Program at Lawrence Livermore National Laboratory.

And, one more, Dr. Surko, Dr. Clifford Surko, who is Professor of Physics at the University of California, San Diego and Co-Chair of the National Research Council Panel on Opportunities in Plasma Science and Technology.

So, without objection, your full testimonies will be included in the record. And, if you would like to summarize and go straight to your main points, that would help facilitate the discussion.

If not, you have five minutes each to present your case. And, then we will move on for questions from the Committee.

And, I guess, Dr. Conn, you will be going first.

STATEMENT OF DR. ROBERT W. CONN, DEAN OF ENGINEERING AND WALTER J. ZABLE PROFESSOR OF ENGINEERING, UNIVERSITY OF CALIFORNIA, SAN DIEGO, SCHOOL OF ENGINEERING AND CHAIR, DEPARTMENT OF ENERGY FUSION ENERGY ADVISORY COMMITTEE

Dr. CONN. Thank you, Mr. Chairman. And, thank you for the opportunity to appear before you and discuss what I think is a very detailed and responsive report on the part of the Fusion Energy Advisory Committee. And, so that's the main reason that I'm here.

I should point out that I was also a member of the President's Committee of Advisors on Science and Technology, the PCAST panel, which was chaired by John Holdren, and produced a report last summer.

I believe that you will find, Mr. Chairman, our recommendations to be very responsive to the interests and issues about fusion energy that were expressed by Congress last year. As you said in your opening remarks, we recommend a new mission for the U.S. Fusion Energy Sciences Program, a new set of policy goals consistent with that mission and a set of principles to enable the program to move forward effectively on that path.

And, we have recommended, I believe, hard choices. And, we have advised on priorities without pulling punches.

So, I would like to say a few things about that and perhaps address the question you made in your opening comment about it seems like the actions that have been recommended are perhaps not as consistent with the overall recommendations. I think, in fact, that they are. And, I will try to explain that.

Our Committee became well aware that the fusion program in Europe and in Japan are now much larger than the programs in the United States. And, I think this was an important part of our considerations.

Europe is spending approximately $600 million a year in the pursuit of fusion energy; Japan, about $450 million. At the budget level that we recommended to the Department of Energy, and that I hope we can ask for your support of, namely $275 million for fiscal year 1997, the U.S. program would be only 20 percent of the world's fusion effort.

And, we believe this is a level close to a marginal one with respect to the U.S. being a credible player on the world fusion scene. And, so because of the program reduction which occurred a year ago and that you referred to as well of almost 33 percent, we would argue that the program should not be cut further.

And, I think you will find that the restructured program is responsive and, therefore, worthy of your support.

I won't go into the background in light of the time. You have summarized it very well, both in your opening remarks, as has Mr. Roemer and others, what the background to the current hearings are.

And, your Hearing Charter was extraordinarily well put together. It really did give an excellent history of the program, a description of the various decisions. And, so I didn't plan to review that.

But, in response, the Department of Energy asked the Fusion Energy Advisory Committee to essentially address the issues that were raised by Congress—how should the program be restructured. And that was the focus of our effort.

I've indicated that both Mike Knotek and Jim Callen are here with me. I do want to publicly acknowledge and thank them for the work that they did, they and the subcommittees.

In the written testimony, I've given a list of all of the subcommittees and a copy of the executive summary of the report. They did this work over a period of six to eight weeks. That included the holiday sessions, snowstorms and everything else that was going on here in Washington.

Most importantly, it was an extraordinarily inclusive process. This is one in which everyone who had something to say was asked to say it. And, it was considered very seriously.

And I believe in the end you will find on balance that what we say in the report is largely the consensus of the community. You never get unanimity, but it's largely the consensus of the community. And, we made a very strong outreach effort.

The other thing that I have included in the remarks for the record are specific answers. There were 14 questions at the end of the Hearing Charter.

And I simply want to indicate that there are written answers provided in the written testimony that I have submitted and which you have accepted for the record. And I won't go through each of them.

I will simply try to make some comments about the highlights. The highlights, to me, are the following. That there are essentially, both in the earlier PCAST report but particularly in FEAC, a set of recommendations that the fusion program adopt a new mission, a set of goals and a strategy where research requirements are determined by the progress that's being made in the sciences and basic technologies of fusion and that underpin fusion energy developed, rather than by the desire and need, or need, to develop fusion power by a specific date certain.

I think that's the fundamental difference between the strategy that we are recommending now and the strategy that was based on, let us say, the 1991 National Energy Policy Act. There, we had a date certain that one wanted to produce fusion energy demonstration. And, it drove the program, rolling back from that demonstration date.

Here, the strategy is to move forward based upon the progress and knowledge plus the scale of the facilities necessary to make progress. I think this is a very important point.

It's our view—and I think it's transparently clear—you cannot make progress in this field of very high temperature plasma physics with good confinement of the energy and heat and particles and so on without facilities of a certain scale size. And, therefore, the program and budget that is required to support such a program also has a certain scale size. And, I believe the budget recommendations we have made to the Department are about the right level.

Let me just summarize very quickly, then, what the new program should be. The mission is to advance science, fusion science and fusion technology, all of which are the base needed for an economically and ultimately an environmentally attractive fusion energy source.

And, as I said, that's not to be done by any specific date certain. And that removes the forcing function on the budgets which you, Chairman Rohrabacher, remarked about earlier in your opening comments.

Now, based on that mission, there are three essential policy goals. The central theme of the U.S. program should be the development of fusion science.

And that means specifically the science of high temperature plasma physics and related areas, basic fusion technology and fusion plasma containment innovations. And innovations can be, I might add, in the tokamak concept or in other concepts.

What one wants to do now is study high temperature plasma physics of importance to the development of fusion energy and use the best means we have to do so. One of the comments I might make in connection with your remark about Larry Lidsky's letter is that if you are not, in fact, pursuing it for a date certain, then what you want to do is understand the best you can.

It has turned out that tokamaks are where they are because they have produced the highest quality fusion plasmas available to be studied. And, if your aim is basic fusion science and understanding, those are very good vehicles for doing that.

The second policy goal is that it should aim to advance plasma science in pursuit of both national science and technology goals. I believe that's consistent with the report of the National Research Council. And, I think Cliff Surko will remark about that.

And, finally, the program should continue to pursue the goal of fusion energy through international collaboration; that is, as a partner in the international fusion effort. And, I do want to note that at the budget levels we recommend, the U.S. is ceding leadership in fusion power development to Europe and Japan.

I think, Congressman Roemer, you are right in that regard. When you spend 20 percent of the money, you haven't a command for the leadership.

You have to, on the other hand, be very smart. And so we have a set of 10 implementing principles in the report. And I just want to summarize them very quickly.

One is the science focus, which I think you know.

Chairman ROHRABACHER. If you could, summarize them as quickly as possible.

Dr. CONN. Okay. Well, then I will close with the following about budgets.

We were asked to look at budgets at the levels of from $200 million to $275 million. And at the constant budget level, which we took to be about $250 million, we feel there are still very painful choices to be made, one of which is that you would be unable to operate—one of which is that you would have to make some hard choices among the facilities. And, perhaps in the question period, I could discuss the issue of TFTR, the DIII-D and the C-Mod facilities.

Much below that level, I believe basically we will be losing any real position in the world program, either in fusion science or in fusion energy development. And, perhaps we could discuss that at the end.

When we looked at what was available to be learned from a scientific point of view, we argued strongly in the end that the budget level of $275 million would permit the program to move on the path of restructuring in a way that I think was consistent with what Congress asked for. So, if I were to summarize briefly, I would say that our report is responsive, not only to the Department's charge to the Committee, but to us.

And, I would like some time when we get to the question and answer period to discuss the issue of why did we recommend certain operation of some of the tokamak machines. I believe it's very consistent. And, with some extra time, I will clarify that point for you.

Thank you very much for the time that you've given me. And we look forward to answering questions.

[The prepared statement of Dr. Conn follows:]

Remarks for the Record to

Subcommittee on Energy and Environment
Committee on Science
U. S. House of Representatives

Regarding

The Fusion Energy Advisory Committee Report of 1/27/96 to the Department of Energy entitled "A Restructured Fusion Energy Sciences Program"

Testimony of Robert W. Conn
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March 7, 1996

Testimony of Robert W. Conn
to
Subcommittee on Energy and Environment
Committee on Science
March 7, 1996

1. INTRODUCTION

Chairman Rohrabacher, members of the subcommittee, thank you for the invitation to appear before you today to discuss the Nation's fusion energy sciences program. For the record, I am the Dean of the School of Engineering at the University of California, San Diego and currently Chair of the Department of Energy's (DOE's) Fusion Energy Advisory Committee (FEAC). I was as well a member of the Fusion Review Panel of the President's Committee of Advisors on Science and Technology (PCAST). That panel reviewed the fusion energy program in the Spring of 1995 and presented its findings and recommendations to the full PCAST at its July, 1995 meeting. The PCAST panel report, entitled "The U. S. Program of Fusion Energy Research and Development", was well received by the PCAST and was released on July 10, 1995. The PCAST panel was chaired by Professor John Holdren and I served as vice-chair.

However, I am here today as Chair of FEAC primarily to summarize the findings and recommendations of a more recent report, issued January 27, 1996 by FEAC and entitled "A Restructured Fusion Energy Sciences Program." For the record, I have attached as Appendix 1 of my written testimony the title page and Executive Summary. The report was prepared in response to a charge given the committee last December by the Department of Energy and responds to the request made by the Congress in the conference committee report for DOE FY 1996 appropriations.

I believe Mr. Chairman that you will find our recommendations to be very responsive to the interests and issues about fusion energy expressed by Congress last year. In particular, the FEAC recommends a new mission for a U. S. Fusion Energy Sciences Program, a new set of policy goals consistent with that mission, and a set of ten principles to enable the program to move forward effectively on its new path. We have recommended hard choices and we have advised on priorities without pulling punches.

In particular, we are explicitly recommending a shift from a milestone-driven fusion power development program to a science-oriented program that will continue progress and innovation while advancing high temperature plasma physics and basic, enabling fusion technologies. This approach is in contrast to our previous objective when fusion power development by a date-certain was the explicit goal of the U. S. program. Now, fusion energy is the long-term goal of the

U. S. program consistent with the mission of the DOE. A key element of the new strategy is to use international collaboration (partnering in the international arena) as the primary means to pursue fusion power development, especially with regard to major facilities.

I should also point out that the fusion program in Europe is about $600 million per year while that in Japan is almost $450 million. At the budget level FEAC recommends of $275 million for FY 1997, the U. S. program will be only 20% of the world fusion energy effort. We believe this level is close to a marginal one in order for the U. S. to be a credible player on the world fusion scene. We believe Mr. Chairman that because the cut in the fusion budget in FY 1996 was almost 33%, the fusion program should not be cut further. I believe you will find the restructured program to be responsive to your requests of last year and therefore deserving of your support and that of the Congress.

2. BACKGROUND

The Department of Energy Charge to FEAC and Congressional Guidance

In December, 1995, Dr. Martha Krebs, Director of the Office of Fusion Energy at the DOE, wrote to me requesting that the FEAC help the Department prepare a strategic plan to guide the Nation and the Department as we move forward with fusion research. She noted in her letter that the appropriations bill passed by Congress and signed by the President for FY 1996 provided $244 million for fusion, a reduction of almost one third below the FY 1995 level of $357 million. Dr. Krebs explicitly noted in her letter that "Congress directed the Department, working with the FEAC, to prepare a strategic plan to implement a restructured program and provide some direction regarding the content of a restructured program".

Further, Dr. Krebs provided the FEAC with a copy of the final FY 1996 Congressional Appropriations Conference Report. In it, the Congress asks that the strategic plan to be developed by the Department "assume a constant level of effort in the base program for the next several years; as appropriate, it should be integrated with the plans of the international program; and it should address the institutional makeup of a domestic program consistent with the funding assumptions."

I summarize this background to make clear that the fusion community and, I believe, the Department of Energy has read the language from the Congress carefully, has listened to the message, and has worked diligently to be responsive to the Congress.

For the record, Mr. Chairman, the FEAC formed two major subcommittees to carry out its work, a Strategic Planning Subcommittee chaired by Dr. Michael Knotek of the Pacific Northwest National Laboratory, and a Scientific Issues Subcommittee chaired by Prof. James Callen of the University of Wisconsin-Madison. I want to publicly and personally thank both Drs. Knotek and Callen, who are here with me today, and all those who served on their subcommittees, for working so hard under very trying circumstances to complete our work. A list of the membership of FEAC and its two subcommittees is given in Appendix 2.

I believe you know, Mr. Chairman, that our work was done during an intense six-week period that included both the holidays and innumerable snow storms. It was the heroic efforts of my colleagues that permit me to be here today with what we believe is a very responsive set of recommendations for you about a restructured fusion energy sciences program for the Nation.

Chairman Rohrabacher's Requests and Hearing Charter

Mr. Chairman, in your letter to me dated February 20, 1996 and in the "Hearing Charter" attached to that letter, you summarized very well the background and history of the program, including the recommendations of the recent PCAST report, and you summarize the recent funding history. You also provide a brief overview of both the charge and the main findings and recommendations of the FEAC January 27, 1996 report.

On pages 6 and 7 of your Hearing Charter, you list 14 questions on issues of interest to you and the subcommittee. Again for the record, I have provided both your questions and a set of written answers in Appendix 3 attached to these remarks. The responses were originally developed for me by the members of the Scientific Issues Subcommittee of the FEAC. As such, these answers represent real fusion community input and I believe are truly reflective of the views of the vast majority of fusion researchers. However, as they are attached here to my testimony and as I participated in the development of the answers, I take full responsibility.

3. SYNOPSIS OF FEAC FINDINGS AND RECOMMENDATIONS ON A RESTRUCTURED FUSION ENERGY SCIENCES PROGRAM.

I understand that you would like me to summarize briefly the findings of the PCAST report of last summer, and then to concentrate on the findings and recommendations of the report prepared by FEAC and released in January of this year. Let me begin by saying that both the FEAC report of this January and the PCAST report of last summer foresee a U. S. fusion energy research program that is sharply different from the one planned by the Department of Energy and the fusion community prior to 1995, and based on the National Energy Strategy of 1991.

Most importantly, both PCAST and FEAC are recommending that the fusion program adopt a mission, a set of goals and a strategy where resource requirements are determined by the progress being made in the sciences and basic technologies that underpin fusion energy, rather than by a desire or need to develop fusion power by a specific date-certain. Further, both PCAST and FEAC foresee a fusion program that is focused on the key scientific and technological issues confronting the development of fusion energy, and less on development issues. Finally, both committees foresee international cooperation as a major key to progress towards the goal of fusion energy.

A New Program Mission

The FEAC itself recommends first and foremost a new mission for a fusion energy sciences program. That mission is

> **"To advance plasma science, fusion science and fusion technology - the knowledge base needed for an economically and environmentally attractive fusion energy source."**

A program consistent with this mission will be fundamentally different than the fusion program based upon the mission of the National Energy Strategy of 1991. In particular the program is not be driven by a plan and a set of milestones aimed at developing a fusion demonstration power plant by a date-certain. Rather, the new restructured fusion program, which we recommend be called the Fusion Energy Sciences Program, will be driven by scientific and technological results, by the scale of experiments and theory necessary to move the field forward in a significant way, by the need to be a credible international partner, and by participation in the international fusion energy program as the means by which we work with the world effort to field a fusion energy source in the first half of the next century.

Three New Policy Goals

Consistent with this mission, we recommend three key, and new, policy goals for the program. First, **the central theme of the U. S. program should be the development of fusion science (specifically, the science of high temperature plasma physics and related areas), basic fusion technology, and fusion plasma containment innovations.** Hence the restructured program will focus on developing fusion science with increased attention to concept improvement and alternative approaches to fusion.

Next, **the program should aim to advance plasma science in pursuit of national science and technology goals.** Plasma science is a cornerstone of the scientific infrastructure of the country, and is a prerequisite competency in the pursuit of many national science and technology goals. The restructured Fusion

Energy Sciences Program of the DOE should undertake the stewardship role for plasma science within the federal government and work with other parts of the DOE, and with other agencies such as NSF, NASA, and branches of the DOD, to ensure a healthy basic plasma sciences research effort in the U. S., consistent with the recommendations of the National Research Council report on plasma science issued in July of 1995.

And finally, but very importantly, **the program should continue to pursue the goal of fusion energy through international collaboration, that is, as a partner in the international fusion effort.** We must recognize that at the budget levels we recommend, the U. S. is ceding leadership in fusion power development to Europe and Japan. We believe the U. S. can and must be a credible player on the international scene and that international collaboration is the best way to maintain a window on worldwide fusion energy developments.

Ten Implementing Principles

In executing the new mission, the committee recommended to the DOE a set of 10 guiding principles. These are:

1. **Have a science focus.** We envision the restructured program to be integrated around a set of national and international experimental that theoretical resource and the interaction of numerous scientific communities.

2. **Have an energy goal.** The new science program serves the DOE's energy mission to enable the development of an abundant, safe, environmentally attractive, and cost-competitive energy source for the future of the country.

3. **Be reliable as an international partner.** In particular, consultation with our international partners should be a major ingredient of the evolution of our commitment to the ITER Engineering Design Activity which runs through 1998.

4. **Play a complementary role in the International effort to develop fusion power.** By complementing the international effort to develop fusion power, following this principle will enable the U. S. to reenter an international effort quickly whenever it becomes nationally advantageous to do so.

5. **Maintain or attain leadership in selected areas.** This will allow the U. S. to make significant progress in fusion science and technology and assure that we are a credible and thus a desirable international partner.

6. **Sustain the highest standards of scientific excellence.**

7. **Aim at an appropriate balance of experimental facilities.** This balance is needed between a few well-integrated, large, national facilities investigating a

spectrum of fusion science and basic technology issues, and smaller facilities more narrowly focused on well-posed scientific problems.

8. **Recognize the importance of a national laboratory for plasma physics.** In particular, the Princeton Plasma Physics Laboratory, the Nation's dedicated laboratory for fusion science, is a critical national resource for the fusion program.

9. **Maintain a strong educational component to the program,** to attract bright and dedicated people to the challenges facing the fusion energy sciences and the development of fusion energy.
And,

10. **Aim at a diversity of participants in the program** to provide a diverse set of intellectual resources to the program.

Mr. Chairman, based on this new mission, this new set of policy goals, and this set of implementing principles, there emerges a new, restructured fusion energy sciences program that we believe meets the objectives of the Congress as expressed last year while also meeting the legitimate needs of this important field of science and energy research.

Budget Scenarios and Implications

Mr. Chairman, the FEAC was asked by the DOE to consider fusion program budget cases ranging from $200 million to $275 million per year. Further, the DOE asked us to take account of the fact that Congress requested the Department to develop a strategic plan based on assuming a constant level of effort in the base program for the next several years while ensuring that the integrated U. S. program is consistent with the plans of the international program. For this reason, we looked first at a "Constant Level-of-Effort" budget of $250 million for FY 1997, and then considered the gains if the budget is $275 million and the loses if the budget were below $250 million.

To move resolutely to a restructured program at the $250 million level, further painful choices must be made. Indeed, painful choices enter even at the $275 million level. After carefully considering the roles and the scientific potential of the three major U. S. operating tokamaks, TFTR, DIII-D, and Alcator C-Mod, the committee concludes that the D-IIID, the C-Mod, and the leading smaller facilities must move toward full, maximally productive utilization. This leads to the painful conclusion that the TFTR must cease operations during FY 1997, running at high utilization for at most part of the year, and at a significant reduction from a full utilization budget. For the lack of resources, we are forced to forego the remaining unique scientific opportunities to study plasma self-heating and reacting plasma phenomena. It is unclear, Mr. Chairman, when these opportunities will come again.

The FEAC recommends maintaining our commitment to the ITER EDA work at the level of effort renegotiated with our international partners last year. The scope of the work we do should be determined in consultation with our international partners. At this time, the International Thermonuclear Experimental Reactor, or ITER, program, is the largest example of an international collaborative efforts involving the U. S. in fusion. It represents the world-wide effort to design a sustained burning plasma machine having the ability to test many of the key technologies needed for fusion power. Here, clear goals and objectives have been defined and the responsibilities of the partners have been clearly agreed to. The Engineering Design Activity phase of the ITER program is due to be completed by the end of 1998.

In our view, even at this budget level, there must be increases for work in plasma and fusion science, particularly on alternatives concepts and advanced tokamak ideas, and the must be modest increases for low-activation materials research and basic fusion technologies such as plasma-materials interactions research and diagnostic development. Finally, we recommend that there be a reduction in the total DOE program staffing, including field offices, so as to have the DOE office scaled appropriately to the new program size and structure.

Mr. Chairman, at this constant level-of-effort budget, there remains continuing and substantial dislocation in this program, which is still adjusting from a reduction of over 33%. At budget levels lower that $250 million in FY 1997, there is a very painful conflict between implementing the goals of the restructured program you requested, honoring our international commitment to the ITER EDA, and obtaining any further valuable scientific benefits from TFTR. At levels significantly below $250 million, the productivity of all major U. S. fusion plasma science facilities would be adversely affected, possibly requiring a reduction to only one major operating facility. opportunities for new scientific initiatives would be severely constrained, defeating the key objectives of the restructuring we recommend. The Nation's technical credibility as an international partner would be damaged further, our ability to meet our commitment to ITER would be further, and severely compromised, and we would not be able to participate in and benefit from the world effort at fusion energy development. We do not recommend this to you.

Rather, Mr. Chairman, FEAC recommended to the Department of Energy, and I recommend to you and you subcommittee, that you provide $275 million in FY 1997 for the newly restructured Fusion Energy Sciences Program. This budget will enable the restructured and redirected program to proceed onto its new course with a purpose and with less destructive consequences. Specifically, a $275 million budget will permit the program to pursue more vigorously the new directions which are at the heart of the recommended restructuring, it will permit us to exploit the highest-priority scientific opportunities remaining at the TFTR facility, it will enable us to strengthen our support of the ITER activity by restoring some of our original commitments to our international partners, and it will permit more productive use of the DIII-D and Alcator C-Mod facilities.

4. CLOSING REMARKS

Mr. Chairman, the committee made a number of other recommendations which you will find in our report. These relate to matters such as program governance, program management, and immediate actions that will help with the restructuring of the program. The FEAC even recommended changes to its own makeup in order that the committee can serve the restructured program better. Hence, I expect some members of FEAC to step down while others are added. It is my understanding that the Department of Energy has already approved a change of the name of the program office to the Office of Fusion Energy Sciences.

In closing, I would like to reiterate that, in my view, the FEAC recommendations are highly responsive both to the DOE charge to my committee and to the requests of the Congress. We have explicitly recommended a new mission, a new set of policy goals and a complete set of guiding principles. We have set priorities and recommended hard choices. The new program represents a significant shift away from a milestone-driven fusion power program to a program more oriented to fusion science and technology. The restructured program will foster progress and innovation in fusion science and basic technology while advancing high temperature plasma physics and enabling fusion technologies.

A key element of the new strategy is the use of international collaboration (partnering in the international arena) as the primary means to pursue fusion power development, especially with regard to major development facilities. I note again that the fusion program in Europe is about $600 million per year and that in Japan is almost $450 million.

The FEAC recommends to the Department of Energy a budget of $275 million in FY 1997 for the U. S. program. Even at this level, the U. S. program is only about 20% of the world fusion energy effort, making us close to marginal as a credible player on the world scene. We believe, Mr. Chairman, that because the cut in the fusion budget in FY 1996 was almost 33%, the new and responsive restructured fusion program should not be cut again. I believe this new program is solid, will serve the Nation well, and meets your requests of last year. It deserves to be supported.

Thank you again Mr. Chairman for this opportunity to testify and to present our views. I'd be happy to answer any questions.

APPENDIX 1

A Restructured Fusion Energy Sciences Program

Advisory Report
Submitted to
Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy

By
The Fusion Energy Advisory Committee

January 27, 1996

Executive Summary

In response to a request from the Director of the Office of Energy Research, this report provides recommendations from the Fusion Energy Advisory Committee (FEAC) on how to restructure the fusion program in light of congressional guidance and budget realities. The restructuring is based on: a survey of the field, including science and technology issues, capabilities, and programs; a new mission for the U.S. Fusion Energy Science program; and a set of policy and science goals. The report includes: the principles and outline of the restructured program; an analysis of the impact of annual budgets ranging from $200M to $275M; and recommended actions to implement the transition and establish a governance system for the restructured program. In this funding range, the United States must concede leadership of the world's fusion energy development effort to Europe and Japan.

The underlying theme of the restructuring is to redirect the program away from the expensive development path to a fusion power plant to focus on the less costly critical basic science and technology foundations. The proposed new mission and supporting policy goals are as follows:

MISSION:* *Advance plasma science, fusion science, and fusion technology — the knowledge base needed for an economically and environmentally attractive fusion energy source.

POLICY GOALS:

- ***Advance plasma science in pursuit of national science and technology goals.***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program.***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

As a first step, we recommend the adoption of the mission and goals and renaming the program the **Fusion Energy Sciences Program**, to reflect accurately the new focus. By incorporating the new mission and goals, the restructured program can fit within a constant annual budget and does not require increased outlays for construction of new facilities.

During the subcommittee process, we identified and assessed eight scientific and programmatic issues involved in the transition to the restructured U.S. fusion program: (1) Fusion Program Scientific Goals, (2) Development of Basic Plasma Science[1], (3) Theory and Computation, (4) Major Tokamak Facilities, (5) Plasma Confinement Research (Alternative Concepts) (6) Inertial Fusion Energy, (7) International Thermonuclear Experimental Reactor (ITER), and (8) Fusion Materials and Technology. Summary assessments are included in the report.

Budget Impacts

The FY96 budget of $244M (a 32% reduction compared to FY95) forced hard choices and has had serious consequences. Looking toward the future, all the funding scenarios require us to close scientifically productive domestic facilities for budgetary, not scientific, reasons, in order to achieve cost-effective utilization of the remaining facilities and to begin the pursuit of new opportunities and directions essential to the restructuring.

The funding level in FY97 is critical, and it is possible only with $275M to move forward briskly on restructuring while accomplishing the full programmatic scope directed in the FY96 Appropriations Report from Congress. Below $250M, it would be necessary to consult again with our international partners on an affordable U.S. share in the ITER Engineering Design Activities (EDA). The restructuring transition would be prolonged and complicated and result in a program that is marginalized in the international context.

In FY98 and beyond, stable funding at or above the FY96 level of effort would allow the United States to remain abreast of international development across fusion science and technology and to continue world leadership in selected specialties. Such niche leadership is essential for us to be sought by international partners as a valued participant, though perhaps minor monetary contributor, for internationally launched major facilities, defining the path to fusion energy production. At all budget levels, an increase in international cooperation (creation of flexible mechanisms to exploit the capabilities of international facilities jointly) is of paramount importance.

Governance

Critical to the success of the restructured program is immediately starting a governance transition, as a mechanism for guiding and implementing the major programmatic changes in a smooth and effective manner.

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[1] **Leadership of the Nation's plasma science research effort is recommended in partial response to the recent Report of the Panel on Opportunities in Plasma Science and Technologies, *Plasma Science from Fundamental Research to Technological Applications*, National Academy Press, Washington, D.C., 1995.**

The **Fusion Energy Sciences Advisory Committee** to DOE's Office of Energy Research, assisted by a continuing **Science Subcommittee**, should advise ER-1 and the program office on policy, goals, priorities, budget, direction, program balance, and governance.

Fusion Energy Sciences Program Management must be reorganized and downsized to match the science-dominated mission, and rely significantly on peer review as the primary input for funding allocations.

Specific programmatic reviews should be conducted and integrated during the remainder of FY96 to help set the technical priorities of the restructured program, given a funding level not to exceed the FY97 President's Budget Request.

- **A Major Facilities Review** in association with a **User Access Working Group**
- **An Alternative Concepts Review**
- Planning for **review of the ITER EDA and its results and to establish criteria for a decision** on future U.S. participation.

The current federal budget realities and the lack of a perceived domestic energy shortage demand program restructuring in accordance with the recommendations in this report, so that the U.S. program will focus on the science and technology foundations for a future or internationally led push toward fusion energy. United States involvement in fusion research and development will continue to be "a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research - - plasma physics - - that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and industry."[2] Enhanced public outreach is needed to keep the public, stakeholder groups, and the broader scientific community fully informed.

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[2] *The U.S. Program of Fusion Energy Research and Development*, Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995.

APPENDIX 2

Fusion Energy Advisory Commitee

Dr. Robert W. Conn (Chair)
University of California, San Diego

Dr. John F. Clarke
Pacific Northwest National Laboratory

Dr. Thomas B. Cochran
Natural Resource Defense Council

Dr. Harold K. Forsen
Retired from Bechtel Hanford, Inc.

Mr. Joseph G. Gavin, Jr.
Retired from Grumman Aerospace

Dr. Katharine B. Gebbie
National Institute of Standards and Technology

Dr. Beverly K. Hartline
Continuous Electron Beam Accelerator Facility

Dr. George R. Jasny
Retired from Martin Marietta Energy Systems, Inc.

Dr. Michael L. Knotek
Battelle Memorial Institute and
Pacific Northwest National Laboratory

Mr. John W. Landis
Stone & Webster Engineering Corporation

Mr. Stephen L. Rosen
Houston Lighting & Power Company

Dr. Marshall N. Rosenbluth
ITER Joint Central Team

Mr. P. Floyd Thomas, Jr.
Lockheed-Martin Energy Systems, Inc.

Dr. James R. Thompson, Jr.
Orbital Sciences Corporation

Dr. Demetrius D. Venable
Howard University

Fusion Energy Advisory Committee
Strategic Planning Subcommittee

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Dr. Harold K. Forsen (Vice Chair)*
Retired from Bechtel Hanford, Inc

Dr. James D. Callen
University of Wisconsin, Madison

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Dr. Michael E. Mauel
Columbia University

Executive Secretary:

Mr. Daniel H. Taft
DynMeridian

Dr. Marshall N. Rosenbluth*
ITER Joint Central Team

Dr. John Sheffield
Oak Ridge National Laboratory

Dr. Demetrius D.Venable*
Howard University

In addition, two other members of FEAC participated extensively in the review:

Dr. John F. Clarke*
Pacific Northwest National Laboratory

Mr. Stephen L. Rosen*
Houston Lighting & Power Company

* Member of FEAC

Fusion Energy Subcommittee
Scientific Issues Subcommittee (SciCom)

Dr. James D. Callen (Chair)
University of Wisconsin, Madison

Dr. Gerald A. Navratil (Vice Chair)
Columbia University

Dr. Patrick H. Diamond
University of California, San Diego

Dr. Earl S. Marmar
Massachusetts Institute of Technology

Dr. Farrokh Najmabadi
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Sandia National Laboratories

Dr. Michael C. Zarnstorff
Princeton Plasma Physics Laboratory

Appendix 3

Responses to Questions and Issues
given on pages 6 and 7 of the
"Hearings Charter"
Subcommittee on Energy and Water
Committee on Science
U. S. House of Representatives

For Hearings Held March 7, 1996 on the Department of Energy's
Restructured Fusion Energy Sciences Program

1. What is the potential role of the Fusion Energy Program in meeting long-term energy needs, and what level of research is justified by that role?

The energy goal of the Fusion Energy Sciences Program is to provide this country and the world with an abundant, safe, environmentally attractive, and cost-competitive new energy source for the future. "Achieving this objective would bring large benefits almost irrespective of how the energy future unfolds; and achieving it could be crucial if society finds it necessary, for environmental or political reasons, to reduce sharply the currently dominant role of fossil fuels in world energy supply." (From the PCAST Fusion Review Panel report, July 10, 1995, p. 5). The PCAST committee went on to identify a real foreign policy concern, namely, that "the United States is unlikely to be able to isolate itself from either the political turmoil or the large-scale environmental problems that will result if it turns out that adequate energy options for countries less well endowed than the United States ... are simply not developed at all, by this country *or* by others." (ibid.,p. 12.) Together, these are powerful arguments for the role of the fusion program in meeting long-term energy needs.

In the view of FEAC, at funding levels in the range of $250M to the recommended $275M, "the United States must concede leadership of the world's fusion energy development effort to Europe and Japan." (FEAC report of January 27, 1996, p. xi). It is nonetheless important to recognize that the development of an inexhaustible and environmentally attractive fusion energy source remains an appropriate high priority in the world. The fusion programs in Europe and Japan are funded at about $600 million and $450 million per year, respectively. In the FEAC-recommended restructured program, the United States program spending will only be about 20% of the total world effort, and the program will focus on maintaining leadership in plasma and fusion science and in basic fusion technology.

We believe this is a marginal level for the U. S. to remain a credible player on the world fusion scene. For these reasons, and because the cut in the fusion budget in FY 1996 was almost 33%, FEAC recommends that the fusion program not be cut further. I believe Mr. Chairman that you will find the restructured program to be responsive to your requests of last year and deserving of your strong support.

2. What is the potential role of the Fusion Energy Program in meeting long-term non-energy needs, and what level of research is justified by that role?

The Fusion Energy Sciences Program has been the largest driver and dominant steward for the intellectual development of the field of plasma science (as noted in both the PCAST and NRC reports). Plasma science is a cornerstone of the scientific infrastructure of this country, and is a prerequisite competency to pursue many national science and technology goals, from national security to industrial processing and astrophysics, requiring major advances at the frontiers of science and numerous enabling technologies. In addition, the fusion program supports the development of a wide range of enabling technologies which have contributed numerous economically significant spin-off applications such plasma processing sources for semiconductor chip manufacturing, ion implantation for machine tool hardening, plasma spray coatings, and high power microwave sources. A large fraction of the activities carried out as part of the core program in fusion sustains the broad field of plasma science and associated technology -- a field whose talent pool, knowledge base, and capability is a national asset of enduring value beyond the specific application of plasma science to fusion energy. Finally, it is the role of the new Fusion Energy Sciences Program to be the "window on world developments" relating to fusion energy development to assure that the U. S. is both knowledgeable and prepared to participate in, or take advantage of, world efforts to develop fusion power.

3. To what extent is the FEAC report responsive to the Congressional directives?

All the Congressional priorities are reflected in FEAC's recommendations for the restructured Fusion Energy Sciences Program. The restructured program assumes a constant level-of-effort for the next several years, as requested by the joint congressional conference report for FY 1996 . As a result, the underlying theme of the restructuring proposed by FEAC is to redirect the program away from the expensive, schedule-driven development of a fusion power plant to a focus on the basic science and technology foundations needed for fusion development. The proposed restructuring is directly and substantively responsive to the Conference Committee Report for the FY 1996 Energy and Water Development Bill, which stated that "the restructured program should emphasize continued development of fusion science, increased attention to concept improvement and alternative approaches to fusion, and development and testing of low-activation structural materials so important to fusion's attractiveness as an energy source."

4. To what extent is the FEAC report responsive to the NRC report?

Implementation of the FEAC report recommendations would be a significant step in addressing the major recommendations of the NRC report. We should clarify that by basic plasma science, the NRC committee and the FEAC mean research on plasmas that are not, or may not, be relevant to the area of high temperature, high density plasmas required in fusion energy systems.

The NRC report calls for a reinvigoration of basic plasma science with approximately $15 million per year, funds that would come from the various federal programs which depend upon plasma science. The FEAC report calls for the restructured Fusion Energy Sciences Program to "...explicitly move to broaden its intellectual and institutional base in fundamental plasma science..., preferably in partnership with other agencies." The FEAC report also envisions development over a several year period of "a fundamental plasma sciences budget in the range of 5% of the funding for the base (fusion) program." The key recommendation is that the Department of Energy's Office of Fusion Energy Sciences provide the stewardship for the science of plasma physics within the Federal Government. The overall support is then to come from OFES in DOE, combined with support from other branches of DOE such as Basic Energy Sciences and Inertial Confinement Fusion, and from other Federal agencies such as NSF, NASA, and the various branches of DOD. In this way, basic plasma sciences can be supported at a total funding level of about $15 million per year.

5. Can cost-sharing through international collaboration be pursued more effectively, and if so, how?

Yes, cost sharing through international collaboration can be a successful strategy for fusion energy development. A prerequisite in our view is a vigorous domestic program in fusion science and technology in order to maintain competency in critical areas and to assure technical credibility as a partner. To be most effective, clearly agreed upon goals and objectives are needed. This will help both to ensure success and to define each partner's responsibilities. Stable funding is also needed to ensure that commitments to long-term international efforts can be fulfilled without compromising our agreement with our international partners or the vitality of the domestic program. At this time, the International Thermonuclear Experimental Reactor, or ITER, program is the largest example of an international collaborative efforts involving the U. S. in fusion. Here, clear goals and objectives have been defined and the responsibilities of the partners have been clearly agreed to. The Engineering Design Activity, or EDA, phase of the ITER program is due to be completed by the end of 1998.

6. What are the prospects that a tokamak fusion reactor will be an economically-viable and environmentally-acceptable commercial electric power producer, and in what time frame?

The prospects for an economically viable and environmentally acceptable commercial tokamak electric power plant are very good. With a rigorous research program, fusion power can produce a substantial portion of U. S. energy needs in the second half of the next century. A recent study by a national team has been performed in conjunction with an advisory group of utility executives, chaired by Mr. Steven Rosen of Houston Lighting and Power. This work identified the chief characteristics and target cost for fusion power plants that will be attractive to the utility industry. Reports of their work are available from the fusion energy research program, School of Engineering, University of California, San Diego. Also, a recent macro-economics study by Battelle Pacific Northwest National Laboratory provided key targets for future fusion plants. Separately, detailed tokamak fusion power plant studies show that these cost targets can be met using advanced modes of operation which have already been observed in operating experiments. Of course, the time when fusion power is actually commercialized depends in part on the financial support provided to the program.

7. What are the perceived advantages and disadvantages of a conventional tokamak reactor?

The world's fusion research programs have focused on the tokamak concept because historically it has produced the best experimental plasmas (hottest plasmas with longest confinement time). The main advantages of the tokamak are that it naturally heats plasma to a high starting temperature via the resistive heating from the current flow, that it has good MHD stability properties, that the plasma shape is readily controlled by external magnetic fields, that tokamak plasmas confine heat and particles very well (indeed better than all concepts investigated to date at medium scale or larger), and that one can produce extraordinary fusion plasma conditions of temperature and density in such machines. These advantages permit the tokamak to be a powerful vehicle for the study of fusion plasma physics, including the physics of burning plasmas. The main disadvantage of a conventional tokamak, however, is that sustained, long-term fusion burn can be obtained only in devices that are large (5 to 8 meters in major radius) and which have some external means of sustaining the plasma current. Machines of 5 to 8 meters in major radius are about the size needed for an actual power plant. This makes the cost of development high.

8. What is the role of ITER, and why does continued U. S. participation in ITER make sense?

The broad physics and engineering challenges that ITER addresses are largely generic to any next step toward the goal of fusion energy. Also, study of the key fusion science issue of burning plasma physics requires the successful integration of all elements of fusion and technology into a large, integrated fusion burning experiment. Burning plasma physics is an important issue because it addresses the feasibility of producing high energy gain plasmas for long times -- it is thus a central issue for practical fusion energy. The cost and programmatic risk for any one country can be reduced by building such a device through international collaboration. The 6-year ITER Engineering Design Activity, which is due to be completed at the end of 1998, is the present international focus of the quest for a burning plasma experiment. Subsequently, if our ITER partners decide to proceed with ITER construction, the United States will have to decide whether it wishes to participate, and if so, to what degree.

As indicated in the FEAC report, continued participation in the ITER EDA and the ITER process is the most cost-effective way for the United States to maintain a strong research effort in burning plasma physics. Furthermore, the ITER EDA will provide a robust and thorough engineering design based on extensive and internationally broad R&D activities. This design has already highlighted a number of significant physics issues and has become a driver in certain experimental and theoretical programs. Finally, continued strong international collaboration and participation in the ITER EDA is the most effective way for the United States program to influence the world development of a burning plasma experiment -- to obtain maximum scientific benefit from such an experiment, and to have our tokamak concept improvements considered in the ITER design.

9. What is the rationale to pursuing alternate concepts as part of a fusion program?

Alternative concept research is required for a scientifically balanced program for two reasons. First, the advancement of fusion science is best accomplished by investigating issues in a variety of magnetic configurations. Though tokamaks have produced plasmas that are most like those needed for fusion power, some key scientific phenomena are more readily studied in alternative magnetic configurations. As one of many examples, the self-generated plasma current that now plays a critical role in the tokamak (the so-called "bootstrap current") was first studied experimentally in a concept, the octupole, which has no prospect to be a power machine. Nonetheless, it proved to be very fertile ground for investigating new phenomena important to fusion science. More generally, the division of fusion science into tokamaks and alternates understates the strong scientific connections between them. Second, the optimal configuration for a fusion power machine is not yet known. It is too early to force a choice between available options.

Investigation of various promising approaches will lead to the best approach, which may draw on properties from several alternative concepts. It has been found over the years that research in a variety of magnetic configurations fosters innovation, creativity, and new ideas -- all of which are also critical to attract new researchers to the field of plasma and fusion science.

10. What is the current status of knowledge for alternate concepts?

All alternative concepts are at a much earlier stage of development than the tokamak (with the exception of inertial confinement fusion, which is under investigation in Defense Programs in DOE). The level of understanding differs greatly from concept to concept. For some of the more speculative ideas, understanding is very primitive. For alternatives that have been studied more extensively, there is fundamental understanding about some physics issues but not a comprehensive knowledge sufficient to project to a power system. In general, the knowledge of plasma science and alternative concepts is sufficiently advanced that well-posed, modest-scale experiments and theoretical studies can be formulated to attack the key physics issues of particular concepts. It is in inappropriate here to specify which concepts are in a particular state of development but proposal review will, as is the normal way in science, determine what work should go forward to further development, and at what pace.

11. What fraction of the fusion budget should be invested in the exploration of alternate and fundamentally different approaches to fusion energy that, at a minimum have the potential of leading to a viable commercial fusion reactor product?

At present, alternate concepts accounts for less than 3% of the fusion energy program, or $7M (not considering inertial confinement fusion research). It is essential that the investment in this research area be increased and that an appropriate scientific balance be achieved between tokamak and non-tokamak research. The present funding includes some physics facilities that are world-class in selected research areas. These facilities are presently underutilized. To benefit appropriately from these investments and opportunities, and to initiate a program in theoretical studies of alternate concepts, the FEAC Scientific Issues Subcommittee estimated the need for an additional $7 million. This would bring funding for concepts alternative to the tokamak to about 6% of the total budget.

Assessment of the appropriate expenditure for new initiatives is difficult since it must be driven by the results of future peer-reviewed proposals. Scientifically meritorious ideas have been advanced which correspond roughly to an expenditure for new experiments in the range of 10% of the current national fusion budget, though again these proposals have not yet all been reviewed, and one would not expect all to qualify for funding. The FEAC foresaw a staged increase of alternate concepts research in the near term to accommodate such ideas. Finally, it is not possible to pre-judge the appropriate funding level in later years since this will depend on actual experimental results and performance.

12. Which fusion plasma physics questions can only be addressed by large tokamak facilities, and which can be addressed by smaller long-term research programs dedicated to isolating and studying fundamental plasma phenomena in a more complete and systematic manner?

To build the scientific foundation for attractive fusion systems, we must understand the physical mechanisms that will enable us to control the performance of plasma confined in a magnetic system. We must understand the plasma edge conditions, the nature of plasma-surface interactions, and the behavior of material surface behavior under plasma bombardment in order to guarantee compatibility between a hot plasma and a practical first wall. We must understand the physical mechanisms that determine the operating limits of continuously-operating, high-performance, and very high temperature plasma devices. And we must understand how burning fusion plasmas behave.

Most fundamental mechanisms can be studied systematically in both and large confinement facilities. The larger ones provide the combination of near-reactor-like plasma conditions, plasma control, and plasma diagnostics needed to address critical scientific questions in the most complete and integrated manner. The smaller devices provide a flexible basis for focused studies of those mechanisms with emphasis on the essential underlying physics. They are also an essential training ground for young scientists.

In short, progress in understanding the physics of fusion plasmas requires sustained research in both large and small facilities.

13. Why is it necessary to maintain operation of three large tokamak facilities?

Each facility has its own unique capabilities that enable it to make distinct contributions to fusion science. The DIII-D tokamak has the world's most advanced set of controls for plasma shape and edge conditions and concentrates on tokamak plasma optimization. Alcator C-Mod's compact size and unusually high magnetic field strength differentiate it from the other facilities and permit it to produce contributions to the world plasma data base that are critical for understanding the relationships between plasma performance and machine parameters. Finally, the Tokamak Fusion Test Reactor (TFTR) is the only device currently producing high performance deuterium-tritium plasmas, thereby providing unique opportunities to study the basic mechanisms of fusion plasma self-heating. The tokamak-improvement programs planned for DIII-D and the relatively new Alcator C-Mod will take several more years to complete. The program of deuterium-tritium plasma studies on TFTR will be completed in about two years and the FEAC recommends that TFTR be operated now to exploit the important, unique scientific opportunities that it provides. The FEAC report recommends operating and completing the TFTR experimental program in FY 1997, and terminating operation during FY 1998, if the fusion energy sciences budget is at or about the recommended $275 million level.

14. Have there been advances in computational techniques that could lead to less reliance on large experimental devices, and if so, are they exploited adequately?

There certainly have been many advances in plasma and fusion science computational techniques and their use in the fusion program increases continuously. One current emphasis is in using predictive modeling to assist in designing precise, effective experiments in order to use most efficiently the limited experimental time available on the major facilities. More generally, since the complete simulation of an entire fusion plasma experiment is well beyond what is possible with even the next generation of massively parallel supercomputers, all calculations of plasma processes are necessarily idealizations of parts of the overall experimental situation. At present, most calculations concentrate on benchmarking key physics aspects against experimental results -- for example, about macroscopic plasma control, plasma turbulence, plasma heat and particle transport, plasma heating, the flow of plasma in the boundary and to the surfaces of the device, etc. Many of these investigations require benchmarking against well-diagnosed, high temperature, fusion-grade plasmas that are only available in the large experimental facilities. Nonetheless, there are many benchmarking investigations, particularly in fundamental plasma physics areas, that can be carried out most effectively in smaller and/or more specialized facilities.

Generally speaking, computational techniques have been exploited adequately in the fusion program. "A robust theory and modeling program" is called for in the FEAC report of Jan. 27, 1996 (see p. 10), in part to ensure continued progress in exploiting computational technique advances.

Chairman ROHRABACHER. Thank you, Dr. Conn. And, we will throw some questions your way about those things.

Dr. CONN. Thank you.

Chairman ROHRABACHER. And, now Professor Drummond.

STATEMENT OF PROFESSOR WILLIAM E. DRUMMOND, FUSION RESEARCH CENTER, UNIVERSITY OF TEXAS, AUSTIN, TEXAS

Dr. DRUMMOND. Based on what Bob Conn has said about the recommendations of this Committee, I would like to address the principal one, which is that this program should now be addressed from a scientific perspective rather than from a development perspective.

As he has described it, for the last 25 years this has been a traditional developmental program in which we assumed the science was known. We assumed there was a date certain and an object certain. And, we proceeded on that basis.

It is plain that in this prospect, in doing so, we made guesses about where science is going to be 10 or 20 years from now. And, then we had the temerity to organize large developmental programs based upon these guesses.

And, as a result, we've had a narrow developmental program with very specific predictions. And, people are now remembering what we predicted 25 years ago. And, that does us no good.

Fusion, in fact, like cancer is a broad, long-term proposition. It requires an understanding of the basic sciences far deeper than we currently have.

And, it needs to be supported like those science programs in this country which are supported for their ultimate long-term application but programs which are recognized as based upon science which we don't understand. For example, the health sciences.

Biology. It is recognized, I think, by everybody that the biological underpinning of science to cure the diseases we would all like to see cured simply doesn't exist.

And, these things are supported because of their expectation that in the long run we will actually get some public benefit from them. And, those programs—for example, the NIH programs—are managed from that point of view.

The fusion program has not been managed from the scientific perspective. And, I would like to discuss the changes in that program that would occur if we actually began to manage the program from a scientific perspective.

And, I think one that has already been brought up is a very good example. In the programmatic development associated with what we've had, large machines are built. But, they are built for specific developmental purposes.

In particular, if we look at TFTR, there is no question the principal motivation of that machine was to demonstrate deuterium/tritium burning in a controlled way. And, although it is a very, very sophisticated scientific machine, this was a developmental objective.

And, there is no question that a good part of that was public relations. We wanted to show people that we could burn tritium and—deuterium/tritium safely.

That being done, the developmental approach would close that machine. On the other hand, if you look at our assets from the point of view of science, that is the most sophisticated, best diagnosed scientific machine in the world with one of the finest scientific teams. But, it hasn't been used for the purpose that it's capable of.

Now, fortunately, in the last six or eight months after it accomplished its deuterium burning phase, it was, in some sense, benignly neglected. And, it began—this scientific team began to undertake experiments which they felt would be important and could be done in this machine.

And, quite dramatically, in the last six or eight months there has been more improvement in the confinement, the confinement of both heat and particles, in the TFTR experiment than had ever been dreamed possible. And, this was a set of scientists working with a scientific machine without the benefit of a developmental objective being forced upon them.

Now, something more has happened. In the last two years—we hear very little about it, but there are theory people, people who actually worry about why things happen.

And, over the last few years, a small team of people have begun to develop a theory—not a theory, a method of calculating what actually happens in these large fusion experiments. As you know, it has been estimated it would take a million times the number of computers in this world to analyze what goes on in TFTR for one second.

But, if you are shrewd enough and clever enough and lucky enough and work hard enough, you can begin to put things together in such a way that you can reduce the computer time to actually get some of these results. Now, this takes years.

It takes the most sophisticated theory. It takes the most sophisticated computing techniques. And, it takes an enormous amount of hard work.

And, there is a group of perhaps six or eight theoreticians, primarily at Princeton and at Texas, who have been working under great pressure not to do this, to try and understand what is actually going on in these large machines. And, about six or eight months ago, they took their results and tried to analyze the actual behavior of the TFTR plasma at Princeton.

Nobody ever started from first principles and said, "Okay, here's a piece of a hardware with so much electricity and so much gas. What's it going to do?" Nobody ever tried to do that before.

And, they tried. They had the temerity to try after six years of work.

And, lo and behold, they were able to predict in some very substantial detail—now, I don't mean to tell you that we understand fusion theory. But, for the first time, from first principles we've been able to actually make some progress in understanding what happens.

And, then, lo and behold, they started analyzing the more spectacular shots at Princeton where they had done these peaks, the so-called super shots. And, their theory still predicted what happened in the super shots; and, moreover, it connected some almost disparate results nobody thought were connected.

They then took this work to England and found the same sort of results. Now, I don't want to suggest to you that we are on the break-through to make fusion power. It's nothing like that.

But, we are at a moment where two things are coming together that are really quite extremely important. Firstly, the recommendation that we treat this as a science program is arriving at the time when science may actually be able to accomplish something in terms of understanding what we are doing.

This—we are on the verge of the most productive four or five years, I believe, in this entire program. And, to do that, we have three major machines, all of which are different, and all of which can be analyzed in this way.

And, we have the universities, which have—there are talented, experimental teams, just as talented as those at the national labs. They are a lot smaller, but they've been shut down for obvious reasons.

We have the ability with just the facilities we currently have, with the theoretical people we currently have, to actually set forth on this great four or five year objective to try and understand the basis of what goes on in fusion plasmas. And, the cost of that, sir, is less than the budget for this year.

That's the science part. I am not addressing the other parts of the fusion program, which the FEAC report does. I'm sticking to the one thing I know a little bit about.

And, for something like $212 million, we can keep all of those facilities working at full along with the theoretical groups. That means Princeton, the TFTR, DIII-D, Alcator C-Mod and the university programs.

We can keep them working at the 1995 level at that kind of cost. And, I believe that's the most productive—that will lead to the most productive five years at no increase in cost every year. These are the most productive five years we will ever have.

And, hopefully, at the end of that five years, we will have some idea as to where we might go in order to produce the original objective. Thank you.

[The prepared statement of Professor Drummond follows:]

TESTIMONY OF PROFESSOR WILLIAM E. DRUMMOND

BEFORE THE
SUBCOMMITTEE ON ENERGY AND ENVIRONMENT OF THE HOUSE COMMITTEE ON SCIENCE

FY 96 has been a difficult year for the fusion program. The program has been faced with major budget cuts and a call for a reevaluation of both its domestic and international programs. In an effort to resolve these matters, the Fusion Energy Advisory Committee (FEAC) has issued a report which develops the outline of a clear public policy which, if properly implemented, could provide the direction the fusion program needs.

The FEAC report proposes a major restructuring of the fusion program which is directly responsive to the congressional mandate on program direction as well as congressional guidance with respect to future funding. The excepts below give the thrust of this proposal.

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"The underlying theme of the restructuring is to redirect the program away from the expensive development path to a fusion power plant (and) to focus on the less costly critical basic science and technology foundations."

> **"MISSION: Advance plasma science, fusion science, and fusion technology - the knowledge base needed for the economically and environmentally attractive fusion energy source.**

This mission and intent can best be met with the following set of policy goals:

- **Advance plasma science in pursuit of national science and technology goals;**
- **Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program;**
- **Pursue fusion energy science and technology as a partner in the international effort.**

These goals have the same major elements as the PCAST key priorities, but emphasize more the science foundation of the program."

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The difference between a science program which has a research perspective and a development program has enormous implications for the management of the fusion program.

A RESEARCH PERSPECTIVE

For the last twenty-five years, the government's fusion program has been a traditional industrial development program with the objective of producing a commercial fusion-powered electric power station by some specified date. The objective was fixed -- only the date changed. The path to that objective was assumed to be known, and the program had only to complete certain specified assignments to reach the objective. The essential physics was taken to be known, and the uncertain physics was deemed to be unnecessary. Experimental facilities were built only to resolve specific development issues; they had no general research role.

The development approach has served the program poorly. Fusion, like the war on cancer, is a broad, long-term proposition for which the scientific base is far from complete. In such circumstances, there is no historical basis for predicting developments even a few years in advance. We have never been able to foretell science. It is fantasy to make predictions for a decade and beyond and unjustifiably pretentious to organize a program on the basis of such predictions. The narrow developmental approach is proving a poor programmatic guide and has led to unrealistic expectations of the program, fusion's great albatross.

The fusion program has made tremendous advances in both knowledge and performance over the last twenty-five years. Despite the progress, we are not yet ready for industrial development of a commercial product. Considerable further advancement will be required before practical applications will be feasible. The program should therefore be organized like other science programs supported for their ultimate practical application. The most familiar examples come from the National Institutes of Health. Everyone recognizes the breadth of biological understanding necessary to cure diseases and the impossibility of predicting precisely which path to a cure will prove most effective.

The best way of encouraging scientific advances is to have good physicists perform good experiments. This is the proven path to scientific progress.

The difference between science and development has immense implications for the fusion program, both philosophical and practical.

Philosophically, we should concentrate our effort on the most problematic, least understood aspects of fusion physics, for example transport. We should emphasize basic understanding and not judge matters solely by the criterion of perceived reactor relevance. Furthermore, we should examine the role of our experimental facilities from a different view. The development program built devices for specific, narrow purposes; having served their purpose, the devices may be closed. On the other hand, a research program builds facilities as the basic resources for experimental investigations. Devices are maintained as long as they are capable of competitive research.

For a long-term, "big-science" area like high energy physics or fusion, the management priorities must be 1) scientific staff, 2) major facilities, 3) specific projects and experiments. All are important, but the ranking is determined by the creation/regeneration time of the resource. (These have not been the priorities of the present fusion program. The first two have been largely ignored and prioritization begun with ranking of specific projects.)

In practice, a research program would be greatly different from a developmental program. For brevity, consider only the two major devices, TFTR and DIII-D, and the smaller, but substantial Alcator C-Mod. These are not only large devices; these institutions are the major reservoirs of experimental expertise. The three devices complement each other in many important aspects. Since we know that machines differ in ways that we cannot yet explain by differences in measured parameters, some diversity of devices is essential to progress, as is the competition between experimental groups and interpretations. For example, although TFTR's contribution to direct reactor development is limited to its unique D-T capability (alpha physics, etc.) -- a role that is nearly complete -- its research capability is far broader. Its high power, excellent diagnostic set, and comparatively simple magnetic geometry provide an important reference point and experimental capability. As we plan for the future, we must maintain the capacity of all of these groups to contribute to fusion science.

Finally, the management required of the Office of Fusion Energy would be greatly reduced -- to the same level as that of other physics research programs in DoE. The management of a scientific program requires very different talents and experience than that of a development program. The head of this program should be a distinguished scientist who has been active in the field, shares the research perspective discussed above, and could insure the scientific integrity and scientific quality of the program. The NIH is a fine example.

THE DOMESTIC SCIENCE PROGRAM

As discussed in the FEAC report, there are a large number of existing and proposed research programs which are potential candidates for support as part of the science program, and the total cost of these far exceeds the funds available. Plainly, with a budget which is substantially lower than in FY 95 and which is expected to remain constant for the foreseeable future, we must develop a set of priorities which allows us to select the absolutely vital science programs and stick with them. Fortunately, the PCAST report provided us with just such a set of priorities and as noted above, these have been endorsed by the FEAC report.

For the past forty years, the principal focus of the fusion research program has been on the physics of fusion plasmas. The goal has been to heat and confine a fusion plasma for a period of time which is long enough for a self-sustained energy producing fusion reaction to occur. By any measure, the results of this research program have been extraordinary. The principal measure of progress, the confinement time, has been improved by more than a factor of a million. To reach the goal of practical fusion power, we need only improve this by an additional factor of five. To take this final step, requires an intense scientific research program sustained for a period of a decade or more. **This is the overriding mandate of the fusion science program.**

To attack this experimentally, we have four major assets: TFTR, DIIID, Alcator C-Mod, and the university programs. TFTR, DIIID and C-Mod are three very different experimental facilities, which are among the best in the world -- all well suited for this research, and all with strong research teams. Due to budget cuts, these are operating at a very low level in FY 96. In addition, there are smaller but equally talented experimental teams in the universities which have had their research facilities abruptly shut down by the DoE in FY 96. Restoring all of these programs to full productivity not only addresses the central problem of fusion science, but is the most highly leveraged use of our limited funds in that it makes maximum use of our previous large investments in both facilities and people. **This is a first priority.**

On the theoretical side, there are outstanding fusion theory groups both in the major labs and in the universities. Together, these give the U.S. world leadership in this vital area. **This also is a first priority and must be maintained.**

Finally, as discussed in the NRC report, there is widespread concern that the support for plasma physics as a basic science has virtually disappeared. Support for plasma physics was historically provided by the DoE, but this has dried up as the DoE fusion program has focused more on development in the last few years. **Broadening the research and teaching of plasma physics within the university community is also a first priority.**

Other programs such as system studies, fusion technologies, material studies, inertial fusion, planning and projects, etc., all of which have already been supported by the DoE, are also necessary if we are to reach the goal of fusion power. However, if we are to fully fund our first priority programs, these other programs will need to be postponed for the present. **These are not first priority programs.**

The details and costs of the first priority projects are given on the following spreadsheet. The first column gives the FY 95 budget breakdown, and the second gives the same budget if TPX, ITER, ITER-related activities, and Germantown overhead costs are eliminated. This does not give a strong research program because the money is not in the right places. But a set of relatively small changes, shown in the third column, gives the strong science program shown in Column four.

The total budget for a strong domestic scientific program is $212.7 million.

Although these changes are small, they are vital. Those which require explanation are discussed on the page following the spreadsheet.

It must be understood that this is not simply an across-the-board cut of the core program. A scientifically sophisticated program management is required to develop and effect this kind of program.

PARTICIPATION IN INTERNATIONAL PROGRAMS

We are currently a partner in the ITER EDA program which is an engineering design study for a proposed very large fusion project. The PCAST report notes that "the ITER Project as currently envisioned, -- combining an ignition experiment with a burn as long as 1000 seconds -- and with a technology-testing phase intended to demonstrate most of the technological features of a reactor -- has led to a project with a price-tag that may be higher than even an international consortium may be willing to pay", and urges that the U.S. renegotiate the goals as well as the scale and timing of such an undertaking. The FEAC report notes that even if the ITER project were to be built, the U.S., with its limited fusion budget, could not be a significant participant. Nonetheless, it is pointed out that we have entered into an international agreement to participate in this design study through its completion in FY 98. The FEAC report indicates that we have, with the agreement of the other partners, reduced our contribution to this project to $55 million per year. It is argued that if we were to stop our participation in the ITER EDA that our reputation as a reliable partner would suffer. As a result, it is argued, this could bar us from future participation in other more scientifically oriented international projects which we might be able to afford and in which we might have a genuine scientific interest.

In the context of a total fusion budget of $275 million, the FEAC report recommends that we spend $55 million on ITER EDA.

DOE OVERHEAD

The staff overhead on the fusion program was $9.6 million in FY 95, but if this is now staffed as a scientific program, this should be reduced to $4.5 million or less by FY 97. Assuming an SBIR charge of $2.75 million, the total DoE overhead should be approximately $7.25 million.

A FEAC FUSION PROGRAM

FEAC indeed presents an attractive proposal which if implemented wisely will profoundly alter the fusion program for the better. The specific implementation discussed above provides for a strong fusion science program at a total cost of $275 million.

FY'97 Domestic Science Budget

	FY95	FY95 x-TPX x-ITER & ITER related	Proposed Changes	FY96 Result
Confinement				
TPX	42.00	0.00		0.00
TFTR	62.60	62.60		62.60
DIIID	51.30	51.30	6.20	57.50
Alcator	16.40	16.40		16.40
PBX-M	4.30	4.30	-4.30	0.00
NSTX	2.40	2.40	-2.40	0.00
International	7.00	7.00	-1.00	6.00
Total Confinement	**186.00**	**144.00**	**-1.50**	**142.50**
D&T		-9.30 *	-6.00 **	
ITER	70.60	0.00		
Plasma Technologies	6.70	6.70		
Fusion Technologies	3.60	3.60		
Advanced Materials	10.20	10.20		
Systems Studies	2.80	2.80		
Total D&T	**93.90**	**14.00**	**-6.00**	**8.00**
Applied Plasma Physics				
Experimental Plasma Research	24.20	24.20	12.00	36.20
Fusion Plasma Theory	17.20	17.20	8.80	26.00
MFE Computing	13.40	13.40	-13.40	0.00
Total Applied Plasma Physics	**54.80**	**54.80**	**7.40**	**62.20**
GPP/Program Direction/SBIR	**18.50**	**0.00**	**0.00**	**0.00**
Inertial Fusion	8.50	8.50	-8.50	0.00
Close Out Costs	3.00	3.00	-3.00	0.00
Grand Total	**364.70**	**224.30**	**-11.60**	**212.70**

* This amount was identified in the Science Committee markup as ITER related and how it is spread among the D&T items is unspecified.

** How this reduction is spread among the D&T items is unspecified.

D&T: -$6 million: This is a case of giving 2nd priority to activities which do not directly support the core physics program.

Experimental Plasma Research, +$12 million: This provides $4 million for the current university programs and $8 million for new university basic research and alternate concept programs.

Fusion Plasma Theory, +$8.8 million: This provides $2.8 million for program wide theory, $2 million for new university programs, and $4 million for a major enhancement of data analysis capability for TFTR and DIIID.

Inertial Fusion, -$8.5 million: This is another case of giving 2nd priority to activities which do not directly support the core research program.

Chairman ROHRABACHER. Thank you very much, Professor Drummond. Sometimes making cuts actually spurs productivity. I think I am hearing that a little bit maybe.

Dr. DRUMMOND. May I comment just on that? I thought last year's cut was the best thing that ever happened to this program, because it got us to talking about what we should do.

Chairman ROHRABACHER. Excuse me. Could you repeat that for the rest of the Committee?

[Laughter.]

Chairman ROHRABACHER. We will go back to that. Okay. And, Dr. Miley.

STATEMENT OF PROFESSOR GEORGE H. MILEY, DIRECTOR, FUSION STUDIES LABORATORY, UNIVERSITY OF ILLINOIS, URBANA, ILLINOIS

Professor MILEY. Thank you, Mr. Chairman. I appreciate the opportunity to present my views to this Committee.

To truly revitalize the magnetic fusion energy program, we need a vision for fusion development, a goal that will serve to inspire not only forefront research but also inspire young scientists and engineers to come into the field. I fear that this vision is currently lacking.

The proposed—the FEAC report, in a sense, talks about a scaled-down present direction program, adding on some somewhat vague science. A new organizational structure is needed to bring together a new generation of fusion scientists aimed at a new vision.

I think the vision is possible, because if you look at the key elements of a fusion reactor, namely confinement, fuel, energy conversion system and the supporting structure, all of these elements have to fit like a fine watch. They have to all work together to complement each other.

And, that would appear to be possible. But, the present reactor concepts don't lead to that vision or goal.

And, in fact, a quite radically different type of reactor needs to be found. I'm not sure we know quite what it is.

An example might be a D-Helium 3-fueled, high Beta system employing direct conversion. And, in the written testimony, I cite a Japanese study of such a plant which turns out to be, I think, one of the most attractive reactor design studies produced to date.

Anyway, the main DOE program doesn't get to a goal like that. It's viewed as evolutionary.

Well, with the change in the budget and the time scale, we need to squarely focus on where we want to go with this vision and how to get there. I think at the moment we have the cart before the horse; that is, we are revising without having an appropriate vision. The change seems to be more reaction to budget constraints, a short-term shift in structure and a commitment to business as usual.

Is this major restructuring needed? Again, I think there are adequate warning signals that have been present for a number of years that say it is.

The original one was the one by Professor Lidsky that you already mentioned. He has maintained that attitude for a long time.

To date, though, the pathfinding fusion reactor studies done by DOE, such as ARIES, Starlite and so on, have failed to reverse that original concern. The Electric Power Research Institute used to participate in fusion research and dropped out some years ago due to concerns.

The realization that the International—well, that ITER would entail a cost and developmental time scale that are not consistent with current budget realities and support is another warning signal.

So, in summary, a basic question is does the revised program, the restructured program, as proposed by FEAC, adequate to resolve these issues and warning signals. I don't believe they will, because they don't get at the root cause of the problems that have caused these difficulties.

The exact way that one would re-engineer the program takes more study. But, let me again repeat. It seems like if you are going to re-engineer a company, you first get a vision. Then, you restructure.

Now, I can't tell you precisely how to do the restructuring. In the written testimony, I've given some thoughts that were based upon earlier thinking.

A number of us talked about particularly the alternate confinement program, which would be a key to getting to this vision. We developed a set of guidelines which were submitted to FEAC. And, they are appended to the testimony.

But, just a couple of points in there deserve note. One is to ensure that this is a serious, effective program, it should have some percent of the budget dedicated to it.

The guidelines that were signed by a number of people indicated that 30 percent of the DOE budget should be specified for alternate approaches at this stage, at least. And, again, progress would determine where that goes later.

The second thing is that since we are searching for the root to this vision, it's too early to narrow options. It's very crucial that we ensure a continual evaluation of new concepts so that despite budget constraints a rollover of funds is provided to examine the new exploratory concepts.

Finally, and equally importantly, and an item that maybe doesn't take that much money is the importance of communication. Since alternate concepts were wiped out by the DOE program, there's no communication among the remaining people who are in this program. The community has been devastated.

Communications needs to be a key item of concern. If we are going to do something innovative, we have to have an exchange of ideas. We have to be able to bring the people together. And, that's not being done now.

I suggested in another letter, which is appended, that a virtual center be set up which would provide this nerve center, sort of think tank type atmosphere for alternate concepts. This would not only include alternate magnetic fusion concepts but also IFE, inertial fusion energy, which is part of the magnetic fusion program at the moment, although it's not magnetic, per se.

This virtual center, combined with a program structure that would ensure the availability of seed money for exploratory concepts and an ongoing rollover of such monies, would pull back together an enthusiastic community of fusion scientists and engineers who are dedicated to the vision of an attractive, competitive fusion system. And, I'm confident that with the proven ability of U.S. scientists and engineers to achieve a goal, once it's clearly defined, that this would revitalize and move forward the fusion program to achieve this vision that we all want and need so badly in the future for future energy.

Thank you.

[The prepared statement of Professor Miley follows:]

G.H. Miley, Testimony prepared for the United States House of Representatives, House Science Committee, Subcommittee on Energy and Environment, WashingtonDC, 7 March 1996

Subject: The DEPARTMENT of ENERGY'S RESTRUCTURED FUSION ENERGY SCIENCES PROGRAM

GEORGE H. MILEY

- Professor of Nuclear Engineering, Professor of Electrical Engineering, and Director of the Fusion Studies Laboratory, University of Illinois, Urbana-Champaign
- Past President, University Fusion Association
- General Chair, IEEE/NPSS *SYMPOSIUM on FUSION ENGINEERING*, 1981 and 1995
- Recipient of numerous honors and awards for fusion research
- Founder and editor of the American Nuclear Society journal, FUSION TECHNOLOGY
- Editor of LASER AND PARTICLE BEAMS and JOURNAL of PLASMA PHYSICS (Cambridge University Press)
- Directed over fifty Ph.D theses concerned with nuclear systems

SUMMARY

To truly revitalize the magnetic fusion energy program, we need a vision for fusion development--a goal that will serve to inspire not only forefront research, but also to inspire bright young scientists and engineers to come into the field. This vision is currently lacking. A generally accepted goal is the development of an economically competitive fusion reactor that has maximum environmental compatibility and safety features. Based on what we know today, that goal could only be achieved by a radical departure from mainline concepts, e.g. by a D-^{3}He-fueled, high-β system, emphasizing direct energy conversion. Yet neither such systems nor the unique base technology needed to achieve them are currently part of the DOE plan. A new organizational structure is needed to bring together a new generation of fusion scientists and engineers, who can enthusiastically focus their creativity on alternative confinement concepts aimed at this goal, on basic research that supports this goal, and on research and development that enables the technology to achieve the goal.

G.H. Miley, Testimony prepared for the United States House of Representatives, House Science Committee, Subcommittee on Energy and Environment, WashingtonDC, 7 March 1996

COMMENTS

My work on the 1976 book, *Fusion Energy Conversion*, sponsored by the Energy Research and Development Administration (the predecessor of the Department of Energy), drove home to me that a fusion reactor must be designed to optimally integrate four key elements:

- the confinement concept,
- the fuel,
- the energy conversion system, and
- the mechanical structure, materials, and auxiliary systems.

Like the gears and pistons in a fine engine, all four components must mesh perfectly in order to work well, or indeed, at all. My view, expressed at the completion of the book, was that "unless all of these elements are put together to provide a very competitive, environmentally compatible reactor by the time we achieve fusion power in 2000, society will not want it." Well, I was wrong--in the year, that is. Now, based on DOE projections, we must substitute the year 2050 for my original prediction! However, the need to develop a finely tuned system--one that will be attractive--will be even more important by the time 2050 arrives, with many new challenges and demands on such energy systems.

However, DOE program projections of present mainline concepts to a reactor fail to provide the synergistic combination of confinement, fusion fuel, energy conversion, and auxiliary systems required to meet this goal. Such a reactor appears to be radically different from present concepts under development by DOE, e.g. one version would be a D-^{3}He-fueled, high-β system employing direct conversion. The present program does not include work on all the key elements involved, much less work on synthesizing these systems into a reactor. A new program should be structured to address this vision, despite the fact that it seems to offer a more difficult challenge than current systems. U.S. scientists and engineers are noted for their ability to meet and conquer technological challenges once the goal is clearly defined. What now appears to be the "easy road" to fusion may, in fact, lead us off a cliff if it is followed unquestioningly.

What has happened to fusion since 1976? Somewhere, the DOE program lost sight of its goal, so that now we face "redirection." Thus far, this redirection is largely focused on how to shut down facilities with minimum pain and how to reinsert some basic science into the program. Unfortunately, I do not see any new vision or excitement in this redirection. I am told that this is because the shutdown costs, combined with the reduced budget, don't allow flexibility yet. In other words, we must wait to determine these new directions until things can be sorted out. Unfortunately, my experience is that unless a clear plan, a vision, a goal is set initially, "sorting out" of this type is likely to be directionless and futile. The successful reorganization of a private company typically comes about by first redefining the company's mission and creating a new vision for the company's future direction, followed by a restructuring of the organization to achieve this vision. We seem to have the cart before the horse in fusion. The restructuring is occurring without a comprehensive new vision; it is more a reaction to budget constraints, a short-term shift in structure, and a long-term commitment to "business as usual."

The question is: "How can OFE judge the validity of its present vision/direction, and how can it decide on new directions?" Industry, when faced with the warning signals of declining revenues and loss of markets, uses tools such as consumer surveys, product line and market share analysis, sales trends, earnings performance, and formal strategic planning, especially relative to special niches where company expertise is strongest. For a government agency--particularly for one without direct "consumers," or even a product to sell--this is more difficult. However, a number of warning signals have appeared about present operations: the early warning by Professor Laurence M. Lidsky, Massachusetts Institute of Technology, that the tokamak concept appeared to him to be leading to a "white elephant" that no one would want; the decision by the Electric Power Research Institute to cancel their small effort in fusion research; the continued failure of the "pathfinding" fusion reactor design studies, ARIES, Starlite, etc., to reverse the original Lidsky concern; the elimination of healthy diversity within the program by cancellation of funding for various alternate confinement concepts; the realization that the International Thermonuclear Experimental Reactor (ITER) would entail a cost and development timescale that are not consistent with present budget realities; and the declining numbers of students electing to pursue fusion R & D in universities.

In summary, the basic question is: "Will the proposed 'restructuring' of OFE, as presently outlined, resolve these issues and address these warning signals?" I do not believe it will, since the root problems remain largely ignored in the current plan. As I stated earlier, the direct approach would be to 1) first identify a new or revised goal for the program, and then 2) restructure the program in a way to best achieve the new goal.

Let me next discuss the "new goal" issue in more detail. Reactor design studies represent a pathfinding exercise that provides insight and an orientation towards a goal. A large number of such studies have been done over the years. I have participated in several and have watched the overall area closely. Today's studies of even the most advanced tokamak reactors lead to results that are only marginally competitive at best. Earlier studies of various alternate approaches have not fared much better. Indeed, the most attractive result by far from a serious fusion reactor study that I know of is one done by Japanese scientists at Nagoya University, based on a D-^{3}He-fueled field-reversed configuration (FRC) reactor, ARTEMIS-L.[1] This reactor design illustrates the importance of the finely tuned engine principle that was stressed earlier. D-^{3}He **fuel** (#1-fuel) was used to minimize induced radioactivity and materials damage; the **FRC** (#2-confinement) was used to get the high-β needed for good D-^{3}He operation and to allow efficient coupling to a direct energy converter; a **traveling wave direct energy converter** (#3-energy conversion) was used to increase efficiency, to reduce the waste heat flow, and to reduce balance of plant costs; and **materials** (#4-mechanical structure) were selected to trade off radioactivity considerations and costs. This was a very serious design effort carried out by a multi-university team of Japanese scientists, supported by engineering from several major companies. The results for a 1000-MWe plant are summarized in Table I.

Table I. Results of ARTEMIS-L D-^{3}He FRC Reactor Study

Capital Cost (1986 $US)	1,800M$
Cost of Electricity (COE)	30.5 mill/kW-h
Public Surface Radiation Exposure during normal operation	<< regulatory requirements
Inherent Safety against LOCA[1]	Yes
Inherent Safety against LOFA[2]	Yes
Minimum LLRW[3] Handling	Yes

[1] Loss of Coolant Accident
[2] Loss of Flow Accident
[3] Low-level Radioactive Waste

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[1] H. Momota, et al., "Conceptual Design of the D-^{3}He Reactor ARTEMIS," *Fusion. Tech., 21*, 2307-2323 (1992). See also H. Momota, et al., "D-^{3}He Fueled FRC Reactor 'ARTEMIS-L'," IAEA, *Fourteenth International Conference on Plasma Physics and Controlled Nuclear Fusion Research,* Würzburg, Germany, 30 Sept-7 Oct 1992; and H. Momota, et al., "Characteristics of D-^{3}He Fueled FRC Reactor 'ARTEMIS-L'," NIFS Report #251, National Institute for Fusion Science, Nagoya, Japan (Nov. 1993).

These results are by far the most attractive found in any serious reactor study, and it is for this reason that I cite this reactor as an example of the "vision" needed for fusion research and development. Why isn't such a reactor a DOE goal? One argument often advanced is that the physics base for such a system is not adequate to allow an evaluation--in other words, the study makes too many assumptions. On the other hand, the study team did make every effort to make the design consistent with all of the existing database. The possibility of achieving such a beneficial goal simply cannot be dismissed by saying the database is lacking. If such a system is set up as a goal, then a corresponding program can be structured to tackle the problem of database and technology development to put together the best combination of fuel, confinement type, energy conversion and auxiliary systems. Such a program would be quite different from that offered by OFE today. First of all, it would be a goal-oriented program with a lofty vision that would bring back excitement and enthusiasm for fusion development. It would of necessity include a strong basic research program to develop the underlying fundamental database required, a strong effort to explore and develop appropriate alternative confinement concepts that could achieve the overall goal, and a forefront technology program to develop the advanced energy conversion and support systems needed.

The exact structure of such a program within DOE and the laboratories needed to accomplish this redirection requires additional study. I can offer some limited advice based on earlier discussions with colleagues in the community and on thinking I did within the context of the need to revitalize the alternative confinement program within DOE. Starting at the November 1995 American Physical Society meeting, I met with a group of fusion scientists interested in this issue, and we submitted the attached "Proposed Guidelines for Revitalization of the Alternate Concept Program" (*Attachment A.*) to the Fusion Energy Advisory Committee (FEAC) and to DOE staff for their consideration. Subsequently, I sent an individual letter (*Attachment B.*) to FEAC and DOE staff titled "A Proposed 'Virtual' Center Approach to Alternate Concept Studies."

Several points made in these letters should be emphasized. First, to insure that new directions are initiated while the restructuring is in progress, some portion of the DOE budget must be identified and dedicated to this effort. Approximately 30% of the total DOE budget was specified as an appropriate target for alternative approaches in the "Guidelines" note. A structure must be developed to bring back together a community of fusion scientists/engineers dedicated to developing the database and design approach to achieve this new goal. With the earlier demise of the base program and alternate approach program, this community has been split apart, with many leaving the field for other more promising or more stable areas of research. Still, many remain, but they are isolated, and there is not an opportunity for effective interactions that are so vital for advancing forefront research. We must regain and build on this community of dedicated researchers if we expect to build momentum and establish national leadership in this area. The virtual center concept, described in *Attachment B.*, provides one mechanism to promote the scientific interaction necessary for such an enterprise. This center would serve as an incubator for new ideas and a clearinghouse for communications and gatherings of key workers in the field. It would function somewhat like a "think tank" or "nerve center" for alternate concept studies. In addition to U.S. work, it would, to the extent possible, incorporate complementary international alternative concepts efforts. This virtual center, combined with a program structure that would ensure the availability of seed money for exploratory concepts and an evaluation plan for more developed concepts, as described in the "Guidelines" attachment (*Attachment A.*), would pull back together an enthusiastic community of fusion scientists and engineers, dedicated to achieving the vision of an attractive, competitive fusion system. A study of how to restructure the magnetic fusion program along these lines is imperative for the long-term survival of the program, and to ensure that our nation is ultimately provided with the attractive and vital fusion power system that is absolutely essential to meet our long-term energy needs.

Attachment A.

To: Fusion Energy Advisory Committee, DOE

From: The Undersigned

RE: Proposed Guidelines for Revitalization of the Alternate Concept Program

Date: 28 November 1995

We all agree emphatically that the alternate concepts fusion program must be restored to allow a vigorous effort in this area and to permit continued exploration of new, innovative concepts. The reasons are clear--as indicated by the Congressional budget message, we need to seek approaches that may offer a faster route to competitive fusion power. Some alternate concepts that received earlier study should be revived if improvements in our understanding or newly developed technology suggest new potential. Fresh, new, innovative approaches should also be aggressively sought. The problem is, however, that it is a formidable challenge to sort through all the possibilities in detail. This will require a significant national effort, but the issue is how to organize an effective effort within present budgetary constraints. The decision about the best approach should be left to FEAC and DOE. In devising such a plan, some guiding principles should be recognized.

We propose the following guidelines:

1) Revitalization of the alternative and innovative concepts program will require a significant increase in the funding designated for this area. In view of the effort required to evaluate new concepts, to expand the database for select concepts, to establish the potential for select concepts, and to initiate next-step studies, an appropriate goal is to increase funding for innovative/alternate concepts to at least 30% of the annual budget as quickly as feasible, the goal being identification of one or more viable, attractive alternates by 2005, as suggested in the draft "Revised Fusion Strategy Plan," unveiled by Dr. A. Davies at the APS Louisville meeting. This budget should include a provision to provide funding on a regular periodic basis to support investigation of new, innovative concepts, and also to provide longer term funding for alternative concepts that have been identified as offering attractive potential for development.

2) Successful alternate concepts development requires a continuity of effort and long-term commitment to attract outstanding scientists. The plan should guard against a tendency to view alternate concepts studies as ancillary to the major national fusion effort, and thus the easiest program to eliminate during difficult funding periods. In fact, even at the reduced budgets envisaged for the future US fusion program, this is the one area where the US could develop a strong, innovative program and, therefore, be recognized as the world leader in this crucially needed endeavor.

3) The optimal alternate concepts have not yet been identified, so it is too soon to narrow research options. The revitalized program should be designed to maintain a broad spectrum of interest and innovation. Provision should be made for "seed money" to explore new concepts, as well as support for studies of more mature concepts. The objective should be to seek concepts that are significant improvements over mainstream devices, not just enhancements.

4) The program structure should provide for a mixture of experimental, theoretical, and systems studies. This would allow a simultaneous expansion of the physics database for select alternates and an evaluation of their potential feasibility or attractiveness.

5) The program structure must insure continual evaluation of new concepts, so that despite budget constraints, a rollover of funds is provided to examine worthy new exploratory concepts. The alternative concepts advisory subcommittee (Item #9) should play an important role in the formulation of appropriate procedures for handling a dynamic program of this type. As more effort is put into concepts identified as promising, it will be necessary to increase the budget for this area. Success would indeed provide strong motivation for Congress to augment the fusion budget and for expanded industrial and utility support of the program.

6) As work progresses, it will be necessary to cancel some studies after initial investigation, in order to pursue others identified as offering more potential. In doing this, every effort must be made to continue the involvement of creative contributing scientists in the community. A key element in program studies should be the identification of critical issues common to a variety of proposed alternates. This will allow the formation of an alternate concept fusion science discipline that would underpin the overall effort and maintain involvement of a small core of key fusion scientists not working on a specific project.

7) The program structure should allow a broad involvement of interested contributors, from a variety of fusion centers, including national laboratories, universities, and industry. The development of collaborative efforts should be a goal, in order to provide a "cross-fertilization" of ideas through interdisciplinary efforts. A structure should be devised to provide close "networking" among scientists working on specific projects, the alternate concept fusion science effort, and international alternate concept centers. In addition to electronic nets, this implies development of appropriate workshops, meetings, newsletters, and support of the archival literature.

8) An important objective should be to develop liaisons with key alternate concepts study groups (both experimental and theoretical) from outside the U.S. A worldwide collaboration is essential in this area due to the variety of concepts and universal tight budgets. To participate effectively in such collaborations, the U.S. must have a healthy program that can provide important input into select areas that supplement or expand the existing international effort. The program proposed here should meet that objective.

9) An effective program will require planning input from both fusion specialists and from knowledgeable physicists and engineers from other fields. A method to obtain such input should be developed; for example, through formation of a subcommittee attached to FEAC. Membership on this committee should be on a rotating basis, to ensure the constant flow of fresh views and to include input from a broad segment of the community. In addition to experts to cover specific concepts of interest at a given time, persons to provide insight into a range of issues, including historical perspectives and interdisciplinary aspects, should also be included. A specific example of a possible structure would be a committee of nine members, of which three are nominated by OFE and six by the alternate concepts community. Each member would serve three years and then rotate out. To launch the committee, one-third of the members will serve for one year, another third for two years, and the remaining third for three years. The committee members would elect a chair and a vice-chair, with the vice-chair assuming the position on the chair's retirement.

George H. Miley, U. of Illinois
Ravi Sudan, Cornell U.
Dalton Schnack, SAIC
James Hammer, LLNL
John Finn, LANL
Dmitri Ryutov, LLNL
Scott Robertson, U. of Colorado
Uri Shumlak, U. of Washington
James Phillips, LANL
Donald Baker, LANL
Peter Cheeseman, NASA
Emilio Panarella, U. Tennessee
Peter Sheehey, LANL
Frank J. Wessel, U. California-Irvine
Robert Krakowski, LANL
Irvin R. Lindemuth, LANL
John Galambos, ORNL
Ronald C. Kirkpatrick, LANL
Chan Choi, Purdue U.
Michael Brown, Swarthmore
Thomas J. Dolan, IAEA
Nick Krall, N. Krall Associates
Paul Moroz, U. Wisconsin-Madison
Stephen O. Dean, FPA
Kenneth Werley, LANL
John R. Cary, U. Colorado
Bruno Coppi, MIT
L. John Perkins, LLNL
J.W. Cobb, ORNL
R.J. Colchin, ORNL
James Degnan, Phillips Laboratory
John Santarius, U. Wisconsin-Madison
Paul Wheeler, LLNL
Rex Gandy, Auburn U.
Gilbert Emmert, U. Wisconsin-Madison
Joe Kwan, LBNL
Cris W. Barnes, LANL
Douglas Keil, Tegal Corporation
Tom Intrator, U. Wisconsin-Madison
Stewart Praeger, U. Wisconsin-Madison

Others (non-US)*:
N. Filippov, Kurchatov Institute, Moscow
A.B. Kukushkin, Kurchatov Institute
Julio Herrara, Instituto de Ciencias Nucleares, UNAM, Mexico
J. Bryan Taylor, UKAEA, Culham Laboratory, UK

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* While this is viewed as a U.S. Program, these individuals participated in the discussions by attending the alternate concept discussion during APS or by being a member of the Alternate Concepts Bulletin Board. The Guidelines have not circulated to other overseas scientists.

Attachment B.

TO:	Dr. N. Anne Davies, Associate Director, Fusion Energy Office of Energy Research U.S. Department of Energy
FROM:	George H. Miley, Director Fusion Studies Laboratory, University of Illinois
SUBJECT:	A Proposed "Virtual" Center Approach to Alternate Concept Studies
DATE:	8 December 1995

Unfortunately, due to OFE's funding constraints, the alternate concepts part of the MFE program has essentially been eliminated, other than for development of improved tokamak concepts. In view of the growing concern about the feasibility of achieving an economically competitive tokamak, it is only prudent to devote a fraction of the budget to alternate confinement concepts. The objective would be threefold: the develop an approach that offers an economically attractive system, a smaller reactor than that possible with a tokamak, and an approach that can capitalize on the advantages of advanced fuels, such as D^3He.

Following a special "informal" meeting on alternate concepts during the APS meeting in Louisville, I circulated via e-mail a Guidelines statement for alternate confinement development. After extensive iteration, about forty scientists signed the Guidelines and it was forwarded to FEAC. Hopefully that document will help by defining basic principles for formation of an effective alternate concept program. However, the specific structure for organizing a program is left open, and the structure poses a real challenge. There are a variety of alternates which, at this stage, range from ideas to concepts that have been studied experimentally. Almost all require more study to evaluate; yet, within the frame of current funding, it is necessary to be selective. This poses a dilemma. In order to establish a viable program with the limited resources available, OFE must develop a thoughtful, aggressive game plan that can be decisive without reestablishing a number of expensive new projects. It is that issue that I wish to address here. My underlying theme is that many of the basic elements (people and equipment) needed for a vital program exist in the U.S. and internationally. However, work is fragmented, communication is poor, and workers are demoralized due to lack of funding. The issue is how to pull this together into an effective program with the mission of identifying one or more viable alternates by 2005.

These conflicting requirements can only be reconciled if an entirely new approach to program management is developed. For that I envision a "virtual" center that serves as an incubator for new ideas and as a nerve center for communications and gatherings of key workers in the field. It would have a physical location and a support staff, but it would not be a standard research and development laboratory. Rather, it would serve as a meeting place, as an information center, and would provide office space and generic experimental facilities for visiting scientists. In this sense it might be compared to a "users" facility where a core of support staff are located to enable work by the users (either working at their home laboratories or visiting the center). The closest analogy I know would be the Laser User's Facility at the UK Rutherford Appleton Laboratory, UK. This facility has enabled laser scientists and students in the UK to do forefront research by supplementing (not replacing their individual laboratory facilities. At the same time, it has served to build a strong interactive community within the field. JET and ITER also have this flavor, but they tend to pull scientists away from home activities to focus on a central facility, whereas the virtual center would retain a "diffuse" think tank character with the emphasis on building on work done at other laboratories.

In addition to serving as a "communication's hub", the center would provide generic computational and experimental facilities. The latter poses a real problem in view of the diversity of concepts of possible interests. A radically different approach would be to design a flexible device capable of experiments covering a range of concepts. For example, field reversed devices, including the FRC, RFP, and spheromak plus the related spherical tokamak, represent a "family" of concepts that are high on the list of alternates deserving study. To study this "family", a basic unit would be designed with the flexibility to cover this entire range of configurations, using a modification of the coil arrangement, etc. to switch between specific experiments. (Indeed, I've described a possible facility of this type in "Compact Tori as Extensions of the Spherical Tokamak," Proc. 6th, ICPP--Research for Advanced Concepts in Magnetic Fusion, Toki-city, Japan, 29 Nov-2 Dec, 1994, *Transactions of Fusion Technology*, 27, 382-386, 1995--however, much more work is needed to establish the feasibility of a flexible facility of this type.) Appropriate experts for a specific concept would be brought in to assist in experiments each time the experiment was reconfigured.

The center's role in computational, theoretical, and design studies would be mainly as a "nerve center" since this work could be done at home laboratories with the center serving to bring the scientists together for workshops, conferences, etc. Appropriate computational facilities would be developed at the center, however, to service visiting scientists and to provide a node on a network including all key fusion computational facilities.

In this manner, the center would encompass most elements of alternate concept research that has been advocated by others, but in a more diffuse way. This is essential because the individual program proposed previously do not appear capable of meeting the challenge posed by the many concepts requiring study but limited money. Concept specific experiments would quickly consume the entire alternate confinement budget and permanently block the evolution of fresh new ideas. Another approach that has been suggested, a computational evaluation center, would sort out the best approaches, based on existing information. However, such an evaluation cannot go far without finding a way to increase the experimental database for a variety of concepts. The virtual center proposed would bring together elements of all these other proposals, but focus on enabling interactions among a wide variety of scientists in an effort to incubate new ideas.

Clearly the center should also strive to be the "communication's hub" not only for U.S. work, but also for an international effort in alternate concepts. With the limited U.S. budget, fostering an international exchange of information is essential in order to build the experimental database needed to evaluate a wide range of alternate concepts. Other countries have a number of experiments of various sizes devoted to various alternate approaches. Unfortunately, the groups tend to be isolated--hampering progress. The proposed center could establish collaborative agreements that could tie together this worldwide effort. The result might be viewed as a miniature ITER-type program.

The location of such a center deserves careful consideration. With the cutback in the programs in a number of DOE laboratories, existing facilities might be found to reduce the expense of establishing a new physical plant. An important criteria would be that the facility offer convenient access and comfortable accommodations for numerous visiting scientists.

More thought needs to be given to this proposal to work out details, and variations of the theme are possible. However, I am convinced that something of this nature is essential under present circumstances. I hope that this suggestions will stimulate discussions that will lead to concrete planning to revitalize this neglected area of the nation's fusion program.

cc: Albert Opdenaker, OFE
Warren Marton, OFE
Ronald Blanken, OFE
Ronald McKnight, OFE
Walter Sadowski, OFE

Chairman ROHRABACHER. Thank you, Dr. Miley. We don't move forward without the vision. And, I appreciate your testimony.

And, I will have a few questions about lack of vision or no vision or wrong vision. And, we will get back to that during the question session.

Dr. Perkins, do have a statement?

STATEMENT OF DR. L. JOHN PERKINS, MAGNETIC FUSION ENERGY PROGRAM, LAWRENCE LIVERMORE NATIONAL LABORATORY, LIVERMORE, CALIFORNIA

Dr. PERKINS. Thank you, Mr. Chairman. Mr. Chairman, I came from England to live and work in the United States about 16 years ago. One great reason I did that was because I was very impressed by the way the U.S. did things—the "get-up-and-go" attitude, the "can-do" attitude, the innovative approach to problem solving and very much the ability or willingness to take risks.

I would like to think that we still lead the world in innovative solutions to mission-oriented problems. I believe we do.

Therefore, I have two related points I would like to make to you today. The first is that a viable fraction of fusion R&D funds should be invested—and, I really stress the word "invest"—in alternative fusion concepts that have the potential of leading to an attractive commercial fusion reactor; and, secondly, that the U.S. should consider adopting this as a leadership role in the world program as one of its primary niche roles—not the primary niche role but one of its primary niche roles.

Mr. Chairman, you talked about fusion earlier. The reason that we are doing fusion is clear to everybody in this program of ours.

And, it's for a very simple reason. Fusion is the only indigenous energy source that will last as long as the earth lasts. And, for that reason alone, it's deserving of a vigorous and sustained research program.

My colleague to my right referred to cancer research earlier. Like cancer research, we've made enormous progress, I think, and understanding in the last 40 years. But, very much like cancer research, we have not come to closure on our final product at this time.

We are very much like cancer research because of the profound benefit to future humanity of a successful fusion reactor. We must continue with innovative thinking and continue to pursue this program.

Let me just go back to the fact that we have made tremendous progress in the last 40 years. Recent achievements in the tokamak research program have also been very great.

And, it does give some confidence that this route to fusion can conceivably produce a functioning power reactor. We certainly should continue that route.

However, it is not clear—that is, it is by no means 100-percent clear that a conventional tokamak reactor alone will lead to a fully practicable fusion power plant that could compete in the energy marketplace of the 21st century. If we are not sure, we should not put 100 percent of our eggs in one basket.

It's also commonly asked whether we will need fusion in the next century. And, to me, the answer is clear.

If you ask me do we need fusion in the next century, I would point out to you that the energy electricity generating business in the next century will be something like a multi-10 trillion dollar industry. So, if you ask me is fusion needed, what you are really saying is. Do we have a sufficiently attractive commercial product that will sell in that marketplace? If we do, then it will be needed.

So, it is my contention that advances leading to a clearly economic attractive fusion reactor will lie in investigating the potential advanced physics of fusion where there are lots of uncertainties, maybe a factor of 10 or more, that we still do not understand rather than simply in engineering the nuts and bolts for the present conventional approach. If we take the conventional approach and engineer it, we are going to get a Model-T Ford. And, what we need is something like, I would say, a Ferrari, perhaps I mean a Toyoto, something that really sells in the marketplace.

And, this requires the promotion of intellectual stimulation in breadth. We've got to encourage parallel approaches with the acknowledgement that perhaps only a few, if any, will succeed.

But, very importantly, we only need one that succeeds and we've cracked the problem. And, it's a supremely good problem.

I would like to comment briefly, if I can, on the Department of Energy's Fusion Energy Advisory Committee, chaired by Dr. Conn. I, like most of my colleagues in this program, support this report in the main.

I would like to, however, just offer two caveats. The first is that FEAC recommended that we restructure to focus on the less costly basic science program.

Now, I will caution that that concentration of basic science—and, by the way, basic science is where these answers are going to come from—that should not be seen as sandbox science or science for science sake. As I suggested above, the advances leading to a more attractive product will come from understanding the physics.

And, so if you were going to do basic science, make sure that that science is directly focused to the commercial reactor power plant. Power plant is what we are in this business for. We must not lose sight of the fact, Mr. Chairman, that first and foremost this is a fusion energy program.

And, secondly, whereas FEAC underlined the need to pursue alternative concepts—and I applaud them for that—they recommended no quantitative budget level. At present, I suggest that the fraction of the fusion budget devoted to alternative concepts is insufficient to promote a critical mass.

I would recommend a budget share of about 25 percent, or of that order, is appropriate and necessary for this activity.

I also note and applaud the fact that FEAC recommended uniform peer review for future investment in new ideas, including advanced tokamak ideas. It's very important that such peer review concepts be concerned with reactor viability.

In other words, if you want money to look at advanced tokamaks or to look at new things, not only have you got to show me why the physics works, of you think it will work, but show me how it makes a better reactor product. How does it boil water for a steam cycle to compete with other things that we expect in the next century?

My final observation on FEAC, as I've said before, concerns the recommendations to secure niche roles in the world program. Why cannot the U.S. lead the world in the pursuance of a vigorous and innovative alternates program?

Make that one of our niche roles after the box thinking that we are very good at in this country.

So, let me conclude, Mr. Chairman, if I can, very briefly. Any breakthroughs leading to a fully economically viable fusion product will lie in the exploration of innovative and alternative physics, both in the advanced tokamak program and especially in new or revisited alternative ideas.

We need out of the box thinking in this program. And, we are capable of it in the U.S. This is where the "science" of the science-based fusion program should be redirected.

And, thirdly, under such a program, it is very important that the physics of a proposed alternate concept be coupled with a reactor embodiment—not science for science sake but science for a better product.

Maybe I could just close with a quotation from a fellow original Brit, Winston Churchill. Winston Churchill said many things, but he also said, "It's no good saying we are doing our best." He said, "We have got to succeed in doing what is necessary," which is an attractive fusion plant.

And, I would recommend that we invest a viable fraction of our budget to those ends. Thank you.

[The prepared statement of Dr. Perkins follows:]

Testimony to the
Subcommittee on Energy and Environment
U.S. House of Representatives
Committee on Science

L. John Perkins
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March 7, 1996

Thank you for the opportunity to appear here today on the subject of the restructured fusion program and the role of alternative fusion concepts. My purpose is to tell you why I believe it is essential to strive for about 25% of the fusion budget to be directed to alternative approaches to fusion energy. I am a physicist in the Magnetic Fusion Energy Program at Lawrence Livermore National Laboratory and have been active in the U.S fusion program for the past fifteen years. I was a member of the first international design team for the International Thermonuclear Experimental Reactor (ITER), and my present research includes the design of fusion power reactors and the exploration of advanced fusion concepts.

Fusion is the only indigenous energy source that will last as long as the earth lasts. For this reason alone, it is deserving of a vigorous and sustained research program. As in cancer research, we have made enormous progress in the fundamental understanding of our field. However, also like cancer research, we have not yet come to closure on our ultimate product. Therefore, by analogy, because of the profound benefit to future humanity of an ultimately successful end point, we must continue with an innovative and, most importantly, diverse research program until that goal is accomplished. Accordingly, my thesis before you today is, first, that a viable fraction of the fusion R&D resources must be invested in alternative fusion concepts that have the potential of leading to an attractive commercial reactor product and, second, that the US should adopt this as one of its primary niche roles in the world fusion program.

The only unlimited, non-fossil options for sustainable, global baseload electricity generation in the long term are fusion, advanced fission and solar-electric. Realizing a successful, economic fusion reactor would be a profound contribution to the well-being and energy security of future humanity, with major global export opportunities in the burgeoning energy industry of the next century. We have made tremendous scientific progress in the world fusion program over the last 40 years or so. Recent achievements in tokamak research give confidence that this route to fusion can conceivably produce a functioning power reactor, and the tokamak will surely continue to be in the forefront of experimental fusion plasma research. However, it is not clear that the conventional tokamak approach alone will lead to a fully practicable commercial power plant able to compete in the energy market place of the next century. This is a consequence of its projected low power density, high capital cost, high complexity, and expensive development path.

It is also commonly asked whether there will be a need for fusion in the next century. To me, the answer is clear: Electrical power generation in the 21st century will be a multi-10-trillion-dollar industry with significant growth in demand predicted for the developing world. Furthermore, if the developing world's demand for power is met by coal and other fossil fuels, we have the potential for a disastrous environmental impact, with major ecological consequences for the entire earth. Thus, what we are really asking is: Do we have a sufficiently attractive fusion reactor product that will sell in this marketplace? If we do, then fusion will be "needed".

It is my contention that advances leading to a clearly economic fusion reactor product will lie in the investigation of potential advanced *physics* solutions rather than simply in engineering the nuts and bolts for the present conventional approach. The smartest investment of our research dollars will be to press for innovation and understanding of the physics of various advanced concepts – because this is where the greatest uncertainties lie and where there is the greatest potential for improving the economics of the fusion power plant. Note also that alternative physics approaches are particularly important if we are to exploit the so-called "advanced" (i.e., non-deuterium+tritium) fusion fuels. The potential advantages of advanced fusion fuels such as deuterium+deuterium, deuterium+helium-3 and proton+boron-11 will likely not be realized in a thermonuclear tokamak, and novel approaches will almost certainly be required.

Thus, at a minimum we must continue to explore advanced physics solutions for the tokamak in an attempt to maximize its ultimate potential as a power reactor. However, we must also vigorously pursue the physics of alternative fusion reactor concepts at a viable level. And, very importantly, we must devote appropriate R&D funds for conceptual and computational development to the stage where new, definitive experiments can be defined. This requires the promotion of intellectual stimulation in *breadth* to encourage parallel approaches with the acknowledgment that perhaps only a very few, if any, may ultimately be successful. However, we only need one. I suggest it is too early and, given the future utility of advanced fission, unnecessary, to put all our eggs in one basket at this stage of fusion development.

It is interesting to define what we mean by "alternative fusion concepts". Table 1 (attached) provides a reasonably complete list of fusion concepts which are, or have been, under study at some level in the world program and classifies them according to their main operating principles.

Other ideas will also surely appear in the future given a sufficiently encouraging environment. The schemes in Table 1 fall into four main classes: (1) Low density magnetic confinement concepts; (2) Inertial confinement fusion concepts; (3) High density magnetic confinement concepts; and (4) Miscellaneous concepts which fall outside the first three groups. Some of these approaches such as the tokamak and standard inertial fusion have a considerable scientific knowledge base. Some others have only been proposed at the conceptual level. Note, however, the fact that such a diverse list of alternative concepts exists does not necessarily imply they are all deserving of funding under an expanded alternatives program. Some may have no reactor relevance while others may have a questionable physics basis.

The class of alternatives in Table 1 under inertial confinement fusion is particularly interesting. These provide routes to fusion power plants which are fundamentally different in many respects from the tokamak, not least offering the facility of liquid walls and the ability to isolate much of their high-technology hardware from multiple, redundant power chambers. In particular, the science of inertial fusion is thoroughly grounded in a strong world effort, while inertial fusion energy research in the US is able to leverage considerable utility from parallel, defense-related programs. Arguably, inertial fusion energy could be considered the primary alternative route and is certainly deserving of an increased budget share under an expanded fusion alternatives program.

Among the recommendations from the recent report from the DOE Fusion Energy Advisory Committee (FEAC) are: (1) That the US fusion program be restructured to focus on the less costly basic science foundation; (2) That future US strategy should be to define and secure niche or supporting roles; and (3) That the concept improvement program be expanded to include a spectrum of alternative concepts. I, like most of my colleagues, am in agreement with the major recommendations of the FEAC report with however, two caveats: First, I caution that concentration on basic science should not been interpreted as "sandbox" science, or science purely for science sake. As above, I suggest that advances leading to a more attractive reactor product will accrue primarily from understanding and extending the physics basis of fusion, both for the advanced tokamak and, in particular, for alternative approaches. Thus, our future investment in the critical basic science foundations *must* be focused squarely at improving the ultimate reactor product. We must not lose sight of the fact that, first and foremost, this is a fusion *energy* program. Second, whereas FEAC underlined the need to pursue alternative concepts, they recommended no quantitative budget level or percentage to this end. At present, I suggest that the fraction of the fusion budget devoted to alternatives is insufficient to promote a critical mass. A budget share of about 25% is appropriate and necessary for these activities.

I also note and applaud the fact that FEAC recommends uniform peer review processes as the primary input to determine funding allocations for new initiatives in the future. It is, however, very important that such peer reviews concern themselves not only with physics viability of a proposed concept but also with reactor viability and, equivalently, prospective development path. That is, to be considered a serious contender for alternative fusion R&D funds, a candidate scheme should be able to point the way to engineering realizations that are a step-change in reactor attractiveness from our present conventional approach. Specifically, this means that the potential for lower capital costs, complexity and development costs should be clearly indicated. Otherwise, why exploit it? This rationale suggests we should be circumspect about funding fusion concepts that have no better reactor potential than the conventional tokamak. An example of this is the stellarator. Although its plasma operation may be qualitatively different from that of the tokamak, its power plant embodiment is really no different in terms of the key attributes of cost and complexity.

My final observation on the FEAC report concerns the recommendation for the US to secure niche roles. Why cannot the US lead the world in the pursuance of a vigorous and innovative alternatives program? This could and should become one of our primary niche roles.

The fusion program should also strive for a unifying element focused on the gathering, generation and objective examination of advanced ideas. The present evaluation of such concepts can be uneven, and ideas tend to be followed in isolation without real appreciation of the underlying potential for power plant development. The assessment of new concepts is typically performed by committee reviews. Experience suggests that such committee processes are unlikely to promote breakthrough ideas that stray from the established path unless accompanied by quantitative supporting analyses. Therefore, the program needs a rational basis to determine which concepts are worth exploring to the next experimental stage. It is my recommendation that a broad, expert team be built to perform physics analysis, configuration design and prospective power plant implementation studies for such ideas. Central use would be made of state-of-the-art computational tools. The primary function would be to provide the results of thorough, quantitative analyses to DOE for their assessment. This could become a cost-effective tool to support DOE in managing an alternates program by taking each idea far enough that they can be down-selected on a rational basis for subsequent definitive experimental programs.

In conclusion, I stress that any breakthroughs leading to a fully economically-viable fusion power plant will lie in the exploration of innovative and alternative physics approaches, rather than in engineering our present conventional route. This is where the "science" of the redirected science-based fusion program should be directed and where a primary niche role for the US should be sought. Equally, under such a program, it is important that the physics of a proposed alternative be coupled with conceptual reactor embodiments to indicate the potential for improvements in capital cost, complexity and development path. That is, based on plausible extrapolations of physics models and experimental data, why should it make a better reactor? If it does not, we should not pursue it. The allure of alternatives to the mainline fusion approach is not new. Over the years, various ideas have been examined both theoretically and experimentally. I recognize that there is no guarantee of ultimate success. Nonetheless, I believe the payoff of a successful alternative to be so high as to warrant a continued and dedicated effort, funded at a viable level.

Table 1. CLASSIFICATION OF ALTERNATIVE FUSION CONCEPTS

LOW DENSITY MAGNETIC CONFINEMENT:

- Standard field-reversed configuration
- Large-orbit field-reversed configuration
- Spheromak
- Spherical tokamak
- Reversed field pinch
- Conventional and advanced tokamak
- Stellarator
- Mirror

INERTIAL CONFINEMENT FUSION:

- Standard inertial fusion (heavy-ion-driven, laser-driven, ...)
- Advanced, fast-ignition systems
- Magneto-inertial concepts
- High-yield pulsed systems

HIGH DENSITY MAGNETIC CONFINEMENT:

- Pulsed Z-pinches (fiber, laser-assisted, staged pinches, ...)
- Plasma foci
- Continuous flow pinches
- Wall-confined, magnetically-insulated concepts

NON-THERMONUCLEAR AND MICELLANEOUS:

- Inertial electrostatic confinement
- Colliding beam systems
- Coulomb barrier circumvention concepts

Chairman ROHRABACHER. Well, thank you very much, Dr. Perkins. Churchill also said, "Never give up. Never, never, never."

Dr. PERKINS. You are absolutely right.

Chairman ROHRABACHER. Of course, he didn't have to worry about the taxes.

[Laughter.]

Chairman ROHRABACHER. I appreciate your testimony. And, Dr. Surko, please go right ahead.

STATEMENT OF DR. CLIFFORD M. SURKO, PROFESSOR OF PHYSICS, UNIVERSITY OF CALIFORNIA, SAN DIEGO, LA JOLLA, CALIFORNIA; AND, CO-CHAIR, NATIONAL RESEARCH COUNCIL, PANEL ON OPPORTUNITIES IN PLASMA SCIENCE AND TECHNOLOGY

Dr. SURKO. I thank you very much for this opportunity to address the Subcommittee. I will briefly describe the recommendations of the National Research Council report on plasma science.

You have copies of it, I think, a blue book, and my written testimony. So, I can only just hit a few key points in this time allotted.

The message I want to stress is the following. That along with technology development, we must do basic science. The present program is out of balance.

And, if action is not taken, such as protecting 5 percent of the fusion budget for basic science, as FEAC recommend, the basic programs will be wiped out by small increases in large projects. In effect, we will have eaten our seed corn for new technology and new development.

The study found that while applications are progressing well, at least many, the underlying fundamental science is not in good health. And, the applications of plasma science are many, not only the quest for useful electrical energy from fusion but lasers, lighting, the cold pasteurization of foods and understanding space plasmas for global communications.

We found that large projects with focused technological objectives have grown to an extent that smaller, more basic activities are dying and dying quickly. And, I should say that the fusion program has been the strongest supporter of plasma science.

And, so it's commitment to plasma science is very important for all applications, not only the quest for fusion but for all other applications as well.

With regard to fusion physics, the report recommended that we should be very careful to make sure that there are a range of project sizes, not just the biggest project one can build. But, in fact, many questions are addressed by smaller projects which permit flexibility in economy in exploring a particular scientific or technical question.

And, if the present trend continues towards only large experiments, a dangerous gap will develop in our ability to address these other important questions.

But, underlying fusion plasma physics and critical to the quest for useful fusion energy is the understanding of basic plasma science. And, of any topic considered in the NRC study, this was the area of greatest concern.

For example, precise studies of ways plasmas can be heated and manipulated are two of the fundamental building blocks that underlie all applications of plasma science. These questions are best studied often in small projects.

And, such activities should be encouraged. However, they have been declining, we found, for more than two decades. And, they have now reached a dangerously low level.

We found, in particular, that there was a critical lack of basic small scale experiments. And, we urge that this be remedied.

There is also a need for coordination of plasma science research. This lack of coordination is apparent in the Department of Energy where there are large programs for magnetic and inertial confinement fusion but no unit in the Basic Energy Sciences Division to provide support for the more fundamental aspects of the science.

The study recommends that the Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, provide increased support for plasma science. Our study also recommended that the National Science Foundation increase its support for plasma science and that the combined Department of Energy and NSF programs provide $15 million per year for basic university-scale experiments.

To further enhance the level of individual investigator and small group activities, we recommend that there be a reassessment made of the relative allocation of funds between large programs and these smaller-scale activities.

I would now like to comment on specific questions posed in the Hearing Charter. And, since the study concluded last year, these are the views of my Co-Chair, John Ahearne, and myself. But, we believe them to be consistent with the report.

Is basic research justified in the Fusion Energy Program? We believe it is.

The path for useful fusion energy is not likely to be an easy one, as has been described here in the last few minutes. Investigation in basic research will continue to be important, not only in achieving fusion energy but in optimizing it for commercial use in the decades to come.

Is there an important role for the Fusion Energy Program in meeting the nation's non-energy needs? Yes, we believe there is.

The fusion program is the largest non-defense user of plasma science. So, a broadly based program, research program, in the Department of Energy could greatly increase the development of all applications. And, I've discussed that range of applications a moment ago.

Is the FEAC report responsive to our study? To quote the report, "The restructured program must explicitly broaden its intellectual and institutional base in fundamental plasma science and technology."

We simply could not agree more. We are encouraged by their recommended policy goal for the program to advance plasma science in the pursuit of national science and technology goals.

The fundamental sciences budget of—the FEAC has recommended that 5 percent of the base program is an appropriate level of funding provided that it's specifically earmarked for the truly basic aspects of the science.

I would like to make just two more points. First, the basic program should emphasize small, university-scale experiments. Just to do some basic science within large projects will not be enough.

Large projects can easily consume all of the available resources. And, if basic projects are done on large devices, then it is the basic science that is most in danger of being dropped if costs rise or near-term priorities dominate.

And, the second point. Judging from experience in other programs, the basic program will need a stable, long term commitment and protection. Otherwise, the small basic program will quite likely disappear quickly with the virtually inevitable cost growth associated with large projects.

So, in summary, the National Research Council study recommends that increased support for the more basic aspects of plasma science be a key element in this restructuring of the Fusion Energy Program. We urge the Congress and the DOE to take this opportunity to create a healthy plasma science that can contribute in a more optimal way to the needs of our society.

Thank you.

[The prepared statement of Dr. Surko follows:]

PLASMA SCIENCE: FROM FUNDAMENTAL RESEARCH TO TECHNOLOGICAL APPLICATIONS

Statement of

Clifford M. Surko
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and
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before the
Subcommittee on Energy and Environment
Committee on Science
U.S. House of Representatives

March 7, 1996

PLASMA SCIENCE: FROM FUNDAMENTAL RESEARCH TO TECHNOLOGICAL APPLICATIONS

Statement of Clifford M. Surko
March 7, 1996

In the mid-1980s, the plasma physics volume of the series *Physics Through the 1990s*[1] signaled problems for plasma science in the United States, particularly with regard to the basic aspects of the science. In the years that followed, there developed a widespread feeling within the plasma science community that something systematic needed to be done to address these issues. Out of this concern, in 1988 the National Research Council created a Plasma Science Committee within the NRC's Board on Physics and Astronomy, a standing committee of the NRC that continues to address plasma science issues. Following establishment of the Plasma Science Committee, plans were begun to undertake a broad study of plasma science and technology. With funding from the National Science Foundation, the Department of Energy, and the Office of Naval Research, the Panel on Opportunities in Plasma Science and Technology was appointed in May 1992 and the study began. The report was approved by the National Research Council in May 1995.[2]

Approximately half of the 13-member panel consisted of experts in the many facets of plasma science considered in this report and half of scientists outside the field, with one of the co-chairs selected as a person with experience in science policy. Three of the members were from industry; one was from a government laboratory and one from an independent research society; and the remaining eight were from academe.

The panel was asked to examine virtually all aspects of plasma science and technology in the United States, including specific areas of plasma science, such as magnetic confinement fusion and beams, accelerators, and coherent radiation sources (called topical areas in the report), and three broad areas, namely fundamental plasma experiments, theoretical and computational plasma physics, and education in plasma science. In this context it was directed to address the following:

1. Assess the health of basic plasma science in the United States as a research enterprise, identify scientific and technological opportunities, and assess potential applications.
2. Identify and address the issues in the field: Evaluate educational programs in light of the nation's future needs. Assess the institutional infrastructure in which plasma science is conducted. Characterize the experimental facilities. Assess manpower requirements. Identify the users of plasma science and their needs.

During the course of the study, the panel took a number of steps to solicit input from the plasma science community. Letters were sent to 200 scientists and engineers, requesting their input on the issues raised in the charge to the panel. A separate, more specialized survey was sent to experimentalists engaged in basic plasma physics research. Input was also solicited from members of relevant scientific and engineering societies by town meetings and by newsletter announcements of the panel's work. There is general agreement from these sources on the themes expressed in the report: There is concern about the decline in basic plasma science, particularly in the area of basic plasma experiments and other small-scale research efforts, and basic plasma science is perceived to lack a "home" in the federal agencies.

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[1] National Research Council, *Plasmas and Fluids*, in the series *Physics Through the 1990s*, National Academy Press, Washington, D.C., 1986.

[2] National Research Council, *Plasma Science: From Fundamental Research to Technological Applications*, National Academy Press, Washington, D.C., 1995.

The panel heard presentations from grant officers involved in funding plasma science. In order to assess the state of education in plasma science, a survey was done of all colleges and universities that had awarded a Ph.D. in plasma physics in the previous five years.

PLASMA SCIENCE IN OUR SOCIETY

Plasma science is the study of the ionized states of matter. Most of the observable matter in the universe is in the plasma state. Plasma science includes plasma physics but aims to describe a much wider class of phenomena in which, for example, atomic and molecular excitation and ionization processes and chemical reactions can play significant roles. The intellectual challenge in plasma science is to develop principles for understanding the complex macroscopic behavior of plasmas, given the known principles that govern their microscopic behavior.

Plasma science is central to the development of fusion as a clean, renewable energy source, which is the focus of this hearing. In order to control the fusion process, which is the source of energy of the sun and the stars, we must learn to create hot, dense plasmas of deuterium and tritium in the laboratory. Great progress has been made toward this goal. Fusion-plasma confinement times have increased by a factor of more than 100 in the last two decades, and achievable temperatures have increased by a factor of 10. There is now in place an international collaboration to design the first prototype fusion power reactor, the International Thermonuclear Experimental Reactor (ITER). It should be noted that the current debate about ITER was prompted by funding mandates that occurred after the panel concluded its work. Thus, the panel did not consider the questions of the relative priority of the domestic program and ITER or other alternatives for international collaboration. In any event, it is clear that continued refinement of the fusion concept and the optimization of fusion as a power source will require reactor-scale projects to test the physics of ignition and burning plasmas. However, progress will also require other research efforts. These projects will be necessary, for example, to improve our understanding of methods of confining and heating plasmas and to develop techniques to lessen the damage to material walls due to the close proximity of the fusion-temperature plasmas. The leverage on investment in this area is tremendous. All major industrial nations have experienced a steady increase in the use of electricity—it is the energy type of choice. Nuclear fission plants are aging, fossil fuels continue to be of concern due to the production of greenhouse gases, and fusion offers the potential of large-scale electricity generation with abundant fuel supply and attractive environmental features.

In addition to the fusion application, plasmas are important to our society in many other ways. Plasmas of interest range over tens of orders of magnitude in density and temperature—from the tenuous plasmas of interstellar space to the ultradense plasmas created in inertial confinement fusion, and from the thermonuclear plasmas created in magnetic confinement fusion devices to the much cooler, chemical plasmas used in the plasma processing of semiconductors.

A healthy plasma science enterprise can be expected to make many important contributions to our society for the foreseeable future. The purpose of the NRC study was to provide guidance regarding the ways in which plasma science can contribute to society and to recommend actions that would optimize these contributions.

Plasma science now impacts daily life in many ways. Low-temperature plasmas, which are hot enough that their atoms and molecules are ionized but cool enough to support a multitude of chemical reactions, are critical to the processing of many modern materials. This method of "plasma processing" is an enabling technology in the fabrication of semiconductors. Important applications include the plasma etching of semiconductors and the surface modification and growth of new materials. A recent National Research Council report[3] highlighted the importance of plasma processing in the electronics industry. Current estimates are that the worldwide sale of plasma reactors alone, which amounted to $1 billion dollars in 1990, are expected to quadruple in this decade. Other important uses of low-temperature plasmas include the "cold" pasteurization of foods, the sterilization of medical products, environmental cleanup, gas discharges for lighting and lasers, isotope separation, switching and welding technology, and plasma-based space propulsion systems.

On Earth, matter is not normally ionized, so plasmas are not readily apparent in our daily lives. Yet plasmas occur in many contexts, spanning an incredible range of plasma densities and temperatures. The most common examples of plasmas that we can actually see are the gas discharges in neon lights, the discharges in bolts of lightning, and the aurorae. Most of the observable matter in the universe is in the plasma state (i.e., in the form of positively charged ions and negatively charged electrons). Plasma science provides one of the cornerstones of our knowledge of the sun, the stars, the interstellar medium, and galaxies. We cannot understand such phenomena as sun spots, the formation of stars from interstellar gas clouds, the acceleration of cosmic rays, the formation and dynamics of energetic jets and winds from stars and galaxies, or the interaction of supernova remnants with interstellar gas, without the concepts of plasma science. Plasmas are central to many aspects of space science. The space plasma medium extends from the ionosphere surrounding the Earth to the far reaches of the solar system. "Space-weather" prediction in the ionosphere and magnetosphere is important for global communications, and the properties of space plasmas are important in determining the capabilities and longevity of spacecraft. Thus, although it is often not readily apparent, plasma science affects our society in a myriad of ways.

CENTRAL MESSAGES OF THE NRC REPORT

The theme of the report is that, although plasmas are pervasive in nature and many of the applications of plasma science are being pursued and exploited effectively, plasma science is not adequately recognized as a discipline. Consequently, there is not an effective structure in place to develop the basic science that underlies its many applications. The potential contributions of plasma science to our society would benefit greatly from a coordinated effort of support for fundamental research, not tied to specific programs but designed to establish this basic scientific foundation.

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[3] National Research Council, *Plasma Processing of Materials: Scientific Opportunities and Technological Challenges*, National Academy Press, Washington, D.C., 1991.

The panel concluded that plasma science is frequently viewed, not as a distinct discipline, but as an interdisciplinary enterprise focused on a large collection of applications. The underlying, common, and critical feature of plasma science as a discipline is that its goal is to understand the behavior of ionized gases, and this requires fundamentally different techniques from those applicable to uncharged gases, fluids, and solids.

This coherence of plasma science as a discipline is apparent when one considers the challenging intellectual problems, central to plasma science, that span applications in many of the topical areas. Examples include wave-particle interactions and plasma heating, chaos, turbulence and plasma transport of particles and energy, plasma boundary layers between plasmas and other materials or between different types of plasmas, magnetic reconnection, and dynamo action.

The report describes many of the developments in theory and experiment that have led to important industrial applications, significant commercial and residential uses, and a deeper understanding of the universe. While delineating the many successes and identifying the exciting and potentially critical challenges, the report is an expression of concern. Applications depend on development, which in turn depends upon a fundamental understanding of the underlying science—the sine qua non of new development. It is the view of experts in the field that this fundamental understanding is, in most cases, best obtained by individual and small-group experimental and theoretical efforts, typical of university-scale programs. The problem is not that there is a gross imbalance in the total funding going into plasma science and technology, but that there has been a gradual, long-term decrease in support for fundamental research in plasma science. The result is that there is now a clear need for such support, particularly in the areas of small-scale, basic plasma experiments and complementary small-group theoretical efforts.

A wide variety of programs pursue the applications of plasma science, including those in space, fusion, and the plasma processing of materials; yet there is great commonality of the underlying science. Even in the context of a particular application, there are often several programs, frequently spread across more than one federal agency. More often than not, this diversity is justified and healthy. However, coordination of research efforts is vital to eliminate duplication, to make the most effective use of resources by maintaining complementary programs, and to ensure that all of the critical problems are being addressed.

A prime example of the existing lack of coordination in plasma research is the fact that *no* agency or agencies have yet assumed the responsibility for basic research in plasma science.

The central messages of the report are threefold:

1. Small-scale research provides much of the fundamental base for plasma science;
2. Such individual-investigator and small-group research is in need of support; and
3. There is a need for increased coordination of federally funded plasma science research.

These conclusions coincide with the principal findings and recommendations of the Brinkman report,[4] which was an overall assessment of the future of plasma physics in the United States conducted a decade ago. If anything, the state of basic plasma science has worsened in the ten years since the Brinkman report was published. This situation can be remedied only by the creation of a coherent and coordinated plan for the support of basic plasma science. The present restructuring of the fusion energy sciences program, which is the focus of this hearing, provides an excellent and timely opportunity to accomplish this goal.

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[4] See footnote 1.

MAGNETIC CONFINEMENT FUSION

A. Scientific and Technical Issues

Magnetic confinement fusion continues to be the largest driver for the development of plasma science. Central to the achievement of fusion in magnetically confined plasmas is the ability to confine hot plasmas (i.e., those with temperatures of more than 100 million Kelvin). Since these plasmas must eventually come in contact with material boundaries, this program also involves important considerations concerning low-temperature plasma science.

Much progress has been made in this field over the past two decades. Confinement times of fusion plasmas have increased by a factor of more than 100, and achievable temperatures have increased by a factor of 10. Progress has been made in the development of new diagnostics of plasma behavior, and these diagnostics have, in turn, led to a deeper understanding of the behavior of fusion plasmas. New methods have been developed to heat fusion plasmas and to drive electrical currents in these plasmas noninductively using intense neutral beams and radio-frequency electromagnetic waves. These methods of current drive could eventually permit the operation of a steady-state fusion reactor. New operating regimes with improved plasma confinement have been discovered, such as the so-called "high-confinement" and "very-high-confinement" modes. There has been progress in the understanding of plasma stability as well as in understanding the interface between the plasma edge and the material walls of the confinement vessel.

One component of the present magnetic confinement fusion program is the design of the International Thermonuclear Experimental Reactor (ITER). There are many plasma processes relevant to controlled fusion that will not be addressed by ITER or similar projects. The physics of the edge plasmas in tokamaks needs to be better understood. Advanced modes of tokamak operation at very long pulse lengths should be studied. This was one of the principal objectives of the now-canceled Tokamak Physics Experiment (TPX) project. Finding improved methods of removing large quantities of heat from the plasma edge is an immediate problem. The efficient production of self-generated plasma currents by high plasma pressures (so-called "bootstrap currents") is an important goal of advanced tokamak configurations that is not likely to be studied efficiently in the ITER program. Pursuing other confinement concepts will also be important, particularly if attempts to control tokamak plasmas fail or become too complex and expensive. Experiment and theory should continue in the search for optimized geometries and operating conditions to improve reactor efficiency and power-handling capabilities.

Crucial to the operation of a fusion device is the transport of particles and energy by plasma turbulence, and turbulent transport has been the dominant transport mechanism in magnetically confined fusion plasmas. There is, as yet, only an extremely limited first-principles understanding of the turbulence in fusion plasmas and the resulting transport. Any predictive capability that does exist is based on empirical "scaling laws" that must be validated when applied outside the operating parameter range of present and past fusion devices. A quantitative understanding of this transport and the ability to control it could potentially lead to improved reactor performance and reduced size and cost.

B. Recommendations

A fundamental base of fusion-relevant plasma science is crucial not only for the efficient development of a successful fusion reactor, but also for achieving a quantitative understanding of fusion plasmas that will continue to be important in maximizing the competitiveness of fusion power in the decades to follow. Thus, the NRC study recommended that, in addition to the research program in basic plasma science described in our general recommendations, there should also be established and maintained a coordinated research program in fusion-relevant plasma physics.

Such a fusion research program will require a range of project sizes, in order to optimize the particular experiments to study the relevant plasma processes. If the present trend toward large experiments were to continue without adequate attention paid to a broader base of experimental research facilities, a dangerous gap would develop in our ability to address the wide range of important questions in fusion-relevant plasma physics. Many of these questions are more appropriately addressed by smaller, long-term research programs dedicated to isolating and studying fundamental plasma phenomena in a more complete and systematic manner. Experimental research is most efficiently done on the smallest scale possible. This allows the greatest flexibility in making changes, as required by new results and discoveries, as well as the greatest exploration of the relevant range of parameters at minimum cost.

A coordinated program of fusion plasma research will require a range of devices and programs—from small, basic experiments that isolate and address fundamental questions in plasma science to experiments on the largest fusion devices. It is also important to maintain a strong parallel program in theory and modeling. It has traditionally been the interaction between experiment and theory that has facilitated the greatest progress in plasma science

The panel envisioned need for two to three small- and medium-scale devices to test innovative confinement concepts. Somewhat larger-scale facilities are desirable to continue basic research in high-temperature (a few keV) plasmas. These devices might function as user facilities, and the panel envisioned need for at least two such facilities. Other questions can be addressed only in larger devices. To study the effects of fusion products (e.g., alpha particles at energies of a few million electron volts) on fusion plasmas, reactor-sized devices, such as the Tokamak Fusion Test Reactor or the Joint European Torus, are required.

BROAD AREAS OF PLASMA SCIENCE

Basic Plasma Experiments. Of all the topics in the NRC study, basic plasma experiments constitute the area of greatest concern. Progress in the physical sciences has relied historically on the close interplay between theory and experiment. Perhaps nowhere is this more true than in many-body physics, which naturally includes plasma physics. Physical phenomena can be identified, isolated, and studied most efficiently, quickly, and economically in experiments specifically tailored for this purpose. There are many advantages of basic, university-scale experiments, compared to larger projects done in settings determined by other considerations such as particular applications. These advantages include the flexibility to choose the setting to isolate a particular physical phenomenon, the ability to explore the broadest range of plasma parameters, and the ability to make experimental changes quickly, guided by the internal logic of the underlying science and by new results as they unfold. Such flexibility is much more difficult in a larger project, and consequently, basic questions should be addressed on the smallest scale that is possible, given the nature of the question being addressed. Such university-scale projects also provide an excellent training ground for research students and postdoctoral fellows and help draw better talent to the field. Despite the importance of basic plasma experiments to plasma science, there have been clear warning signs for more than a decade of a deficiency in this area. This was expressed clearly in the Brinkman report, *Physics Through the 1990s*, written a decade ago. The finding of the NRC study was that this situation has worsened since the Brinkman report was issued. There are several causes of this problem, which include the narrowing focus of the large applied programs such as fusion and space applications. The future health of plasma science as a discipline hinges on the revitalization of basic plasma science, particularly the revitalization of small-scale basic plasma experiments—the area of most rapid decline in the last 20 years. Plasma science is suffering from application without replenishment: With major emphasis on applying what is known and without maintaining the basic scientific effort, the "seed corn" is quickly disappearing. To reinvigorate experimental plasma science in the most efficient and cost-effective way, the NRC study concluded that emphasis should be placed on support for university-scale research programs. This conclusion is based on two findings: Many if not most of the important outstanding problems in basic plasma science can be addressed by this mode of experimentation. In addition, where small and individual principal-investigator-led programs are possible, the degree of flexibility, diversity, and creativity associated with this mode of research optimizes the limited resources that can be expected to be available for basic plasma research in the foreseeable future.

Theory and Computational Plasma Physics. Complementing experimental observation, theory and computation are critical components of modern plasma science. The theoretical problems in plasma science are formidable. The goal is to achieve quantitative understanding of nonlinear, many-body phenomena in these non equilibrium systems. Future challenges to theoretical and computational plasma physics arise from several different considerations. The fact that there is a vast range of practical applications of plasmas—from fusion, to plasma processing, to new radiation sources and accelerators, to global communications—requires not just a qualitative but a quantitative understanding of the underlying plasma behavior. One particularly important and broad topic in theoretical and computational plasma science is the achievement of a deeper understanding of plasma turbulence and the associated transport that frequently results from it. Another broad area of fundamental importance is understanding the evolution of currents, flows, and magnetic fields in plasmas. Such phenomena are important in applications ranging from transport and disruptions in tokamaks to low-temperature plasmas for materials processing and welding to space and astrophysical plasmas. While the present programs of theory and modeling have been successful, particularly in the context of the large applied programs, the NRC study concluded that two modes of research need to be reemphasized. There is a need for individual-investigator-led research on questions fundamental to basic plasma science, such as stochastic effects, novel analytical techniques, and a variety of nonlinear processes. Emphasis also needs to be put on pursuing the commonality of physical processes and mathematical techniques across the many subdisciplines of plasma science.

Education in Plasma Science. Plasma science is a fundamental scientific discipline that has made and continues to make significant contributions to our society. The field is intrinsically interdisciplinary, much like modern materials science. Yet, there needs to be a focus and home for plasma science in modern educational and research institutions for the field to develop properly and have the maximum impact. The NRC study found that plasma scientists are less likely to be in tenure-track positions than are other physicists. Presently, courses in plasma science are unavailable at many educational institutions. If this trend continues, plasma science education and basic plasma science research are likely to decrease in both quality and quantity. The entire field of plasma science, including its educational function, will be strengthened by the study's recommendation that increased support of basic plasma science and university-scale research be provided by the Department of Energy and the National Science Foundation. The programs supported in this way can be expected to attract and maintain the scientific talent necessary to provide high-quality educational programs in plasma science and technology.

GENERAL CONCLUSIONS AND RECOMMENDATIONS

Ongoing research and development programs in the United States have produced important advances in plasma-related science and technology. Plasma science holds promise of further progress in the future, including progress toward the achievement of useful, fusion-generated electricity, advanced methods of processing materials, better methods for cleaning up environmental hazards and mitigating the effects of deleterious chemicals, new methods of accelerating particles and producing electromagnetic radiation, and improved understanding of our space environment and the astrophysical media of the universe. Thus, plasma science can have a significant impact on many disciplines and technologies, including those directly linked to industrial growth. This impact, however, is critically dependent on the support of basic plasma science. It is important to effect some shift of research funds to this area because of its close relation to applications. To properly pursue its potential, the United States must create and maintain a coherent and coordinated program of research and technological development in plasma science. Currently, support for basic plasma science is mostly for small programs, found in many agencies, and not coordinated among agencies. The Department of Energy, for example, has large programs for the development of fusion energy, by both magnetic and inertial confinement schemes, but it has no unit in the Basic Energy Sciences Division to provide support for the study of the fundamentals of plasma science. The National Science Foundation (NSF) is viewed as the supporter of basic science in universities, and there are several quite small plasma programs scattered throughout the agency. However, basic plasma science has no identified home in NSF and, thus, no specified coordinating and review point and no sponsor. The NRC study recommended increased support of university-scale experimental research in basic plasma science in the amount of $15 million per year. The justification for this amount is discussed in the report. Although it may seem that this could have only a small influence on a field with an annual budget in excess of $400 million, the expenditure on other than the two largest applications, fusion and space plasmas, is less than 10% of this amount. Consequently, an investment of $15 million on basic experiments can be expected to provide an important stimulus to the entire field. It can also be expected to have a multiplicative effect in that the results in basic plasma research will provide the foundation for research more closely related to all of the applications, including space and fusion. While many successful programs in plasma science are currently under way, there is a lack of support for the basic aspects of plasma research, particularly where the payoff to a specific program cannot be justified in the near term. The development of plasma science would be improved substantially by its recognition as a scientific discipline.

Given these findings and conclusions, the NRC study recommended the following actions which are relevant to this hearing:

1. To reinvigorate basic plasma science in the most efficient and cost-effective way, emphasis should be placed on university-scale research programs.
2. To aid the development of fusion and other energy-related programs now supported by the Department of Energy, the Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, should provide increased support for basic experimental plasma science. Such emphasis would leverage the department's present investment in plasma science and would strengthen investigations in other energy-related areas of plasma science and technology.
3. To ensure the continued availability of the basic knowledge that is needed for the development of applications, the National Science Foundation should provide increased support for basic plasma science.
4. Approximately $15 million per year for university-scale experiments should be provided, and continued in future years, to effectively redress the current lack of support for fundamental plasma science, which is a central concern of the study. Furthermore, individual-investigator and small-group research, including theory and modeling as well as experiments, needs special help, and small amounts of funding could be life-saving. Funding for these activities should come from existing programs that depend on plasma science. A reassessment of the relative allocation of funds between larger, focused research programs and individual-investigator and small-group activities should be undertaken.
5. The agencies supporting plasma science should cooperate to coordinate plasma science policy and funding.

During the course of the study, the panel had numerous discussions about the desirability of establishing organizational units specifically devoted to plasma science in the relevant federal agencies. Many members of the plasma science community who were consulted strongly advocated the establishment of such homes, believing that they are needed if basic plasma science is to be given the focused attention and increased support that the study recommended. While this subject was considered to be beyond the scope of the panel's work, the panel did suggest that the federal government give this issue further consideration.

QUESTIONS POSED IN THE HEARING CHARTER

The preceding is a summary of the NRC report, focusing on those aspects which are relevant to this hearing. In addition, the Chairman has requested that specific questions be addressed. The panel did not specifically address these questions, since it disbanded once the report was approved by the National Research Council. Thus, the following are the views of myself and my co-chair, Dr. John Ahearne. However, we believe they are consistent with the NRC report.

Controlled fusion offers the possibility of meeting the energy needs of the United States and other developed countries for many centuries. However, the path to the economic generation of electricity using fusion energy remains a long and difficult one. This path is by no means clear of major obstacles—some we see now and others which will undoubtedly arise. Investment in basic research has been, and will continue to be, the key to overcoming many of the problems which will undoubtedly be encountered.

Traditionally, magnetic confinement fusion has been the largest user of plasma science, and it has driven much progress in the field. If properly configured, a broader program in plasma science, conducted in cooperation with the Magnetic Fusion Energy Program, could increase greatly the contribution of plasma science to society in many other areas, while shoring up the fundamental underpinnings of fusion plasma physics.

The FEAC report is responsive to the NRC report's key recommendation to reemphasize basic plasma science research. To quote the FEAC report:[5]

> "As the centerpiece of the nation's plasma science infrastructure, the restructured Fusion Energy Science Program must explicitly move to broaden its intellectual and institutional base in fundamental plasma science and technology."

FEAC recommends that one of the three policy goals of the restructured Fusion Energy Sciences Program be to "advance plasma science in pursuit of national science and technology goals."[6] The FEAC report makes a significant step in the direction of remedying the need for increased funding for basic plasma science, as called for in the NRC study, by specifically recommending a fundamental plasma sciences budget in the range of 5% of the funding for the base program.[7]

In our view, there are two issues that should be addressed in implementing the FEAC recommendations:

First, the basic science program must be protected. It is small but it is critical, and experience has shown that, given pressures on such a large development program, enormous fluctuations can be created in associated smaller programs. In large programs—and the fusion program has demonstrated this in the past—unexpected cost growth and budget pressures can squeeze out smaller programs, especially those with a long time horizon. Projects that take a year or two to plan, and several to construct and get to the point of getting results, can be destroyed when there is a year or so gap in funding. Among other consequences, such gaps destroy the continuity of research teams involving junior personnel such as students and postdoctoral fellows. Basic research is not like constructing a building, in which construction can be resumed relatively easily after a year or two of work stoppage. Basic research, as Vannevar Bush reminded Washington more than fifty years ago, is a slow voyage on an uncharted sea.

The second remaining issue is to make clear that the $15 million recommendation for plasma science experiments funded by DOE and NSF must be specifically earmarked for the most important basic plasma science questions. Given the relatively small amount of resources that one can devote to this task, the NRC panel felt that the most critical need is basic university-scale experiments. In order to use the money most effectively to meet this critical need, the panel concluded one must invest in the smallest experiments capable of addressing the issues of interest. Small (i.e., university scale) experiments are by far the most economical (and they provide the best training ground for research students). Care must be taken to focus on the key basic issues—it will not be nearly as effective to address some basic issue in a larger device and count it as an expenditure on basic research in the context of this recommendation. The relative cost can easily be a factor of two or three different for the same science, and priorities of machine time can further degrade the effectiveness of the project.

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[5] A *Restructured Fusion Energy Sciences Program*, Report of the Fusion Energy Advisory Committee, U.S. Department of Energy, Washington, D.C., January 1996.

[6] See footnote 5.

[7] See footnote 5.

With regard to the scale of experiments necessary for fusion plasma science, one recommendation of the NRC report was that it is necessary to keep a range of devices, so that experiments on fusion plasma science could be conducted in the most cost effective and complete manner possible. This will be a continuing concern. One or a few large projects can eat up all of the available resources. A fluctuation in such a large program can be an entire budget for a smaller device. There must be a system for continued review of this issue to protect the true basic science component.

Regarding the role of computation in plasma science, it is clear that computation has an important role to play. It is complementary to experiment but cannot replace it. We are far from simulating the operation of a tokamak, but great progress has been made in addressing key issues. Experiment is needed to benchmark computational procedures. Moreover, physics is an experimental science. For example, the enhanced confinement regimes and understanding how to accomplish radio-frequency heating and current drive were experimental discoveries. For the foreseeable future, computation will be an important tool for plasma science, but is not likely to be able to replace properly designed experiments. In this regard, computation is likely to play an increasingly important role in the design of experiments, particularly large ones.

SUMMARY

The NRC report calls attention to the critical state of basic plasma science in the United States and the benefit to our society that would follow from an investment in this area. The current restructuring of the Fusion Energy Sciences program provides an unparalleled opportunity to make a major step in this direction and to reinvigorate fusion plasma physics research, which is also recommended in the NRC report. The FEAC report is in support of this course of action. We urge Congress and the Department of Energy to make optimum use of this opportunity—to provide increased support for the more basic aspects of plasma science which provide the foundation for the many important applications and potential applications of plasma science.

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Chairman ROHRABACHER. Dr. Surko, you were just—so, you agree that 5 percent is the level of basic research that you think is an appropriate figure?

Dr. SURKO. We think that's an appropriate level, but it's very careful how that's defined and used. And, so we expect that that would be for the truly basic questions in plasma science, not an augmentation of applications.

So, it's very careful how the structure is set up and how that money is protected. But, yes, at that level, the NRC study recommended $15 million for basic university-scale experiments, but we had in mind small-scale activities.

It would be easy to do some basic science on TFTR and eat up $3 million or $4 million in just the hardware and then not have the time to pursue it because of competing priorities, for example.

Chairman ROHRABACHER. And, you said that 5 percent should be, as you just highlighted, for smaller projects rather than being part of a larger project, because it would be less likely to be cut out.

Dr. SURKO. That's correct, sir. The small projects and basic was plasma science issues.

Chairman ROHRABACHER. Yes. Well, thank you very much.

There seems to be a little disagreement here, and I would like to focus on this for a moment as we talk about basic research. Professor Drummond seems to think that we should not have—or, not making a rule for the whole program but that basically we should be saying that if you have a goal-focused research you are not going to get as much out of it.

But, I think that Dr. Perkins was saying we should have goal-oriented research. Am I getting this correct here?

Dr. Drummond.

Dr. DRUMMOND. Yes. I think—I'm not sure what you understood me to say, but what I meant to say was that the science base needs to be—we need to understand the basic science first and that applications and potential criteria associated with reactors are definitely not part of that activity.

And, perceived reactor relevance should not be a part of our basic research program.

Chairman ROHRABACHER. Okay. I understand that. Now, Dr. Perkins, I sort of felt that that was contradictory to the approach you were suggesting.

Dr. PERKINS. Mr. Chairman, this is a fusion energy program. And, we are looking for an energy product.

I think that's clear. And, I think everyone would agree with that ultimately.

If there are advances to be had in that product, they are going to come from understanding the basic science and innovating sciences. There are factors of maybe up to 10 uncertainty, maybe more in some advanced concepts, in just what the basic underlying physics is.

So, that's where it's going to come from, the basic science of understanding advanced tokamaks and alternative concepts. But, you must keep one eye on the ultimate product and you must not do basic science if it's not squarely focused on that final goal if you are part of the fusion energy, I believe.

Chairman ROHRABACHER. Well, I'm torn a little bit by some of this talk about whether it should be—the focus on basic science. Obviously, Dr. Surko is talking—is saying that we should have—I mean, 5 percent is not a lot of—a high percentage of having a focus on basic science. That is not a high percentage.

But, I do realize, after looking at this program over its history, that you could spend, you know, enormous sums of money just on basic science without ever coming to any conclusion and without ever seeing anything result from that that would produce energy except maybe stacks of paper that can be burned. And, so I think that's a trap as well.

Dr. SURKO. There is a difference here between the two testimonies, if I might just comment on it. For a small part of the program, the underlying basic plasma science can impact all applications.

And, it's important for the Fusion Energy Program to be a part of that, because they are a user of it. So, understanding there are many well-posed questions such as how energy is transported out of a plasma that can be useful for a range of applications, it's important for our nation, we think—the National Research Council study thinks—that for there to be a contribution from the Fusion Energy Program towards that because they benefit from it.

Chairman ROHRABACHER. And, Dr. Miley, you had something?

Dr. MILEY. Not to confuse the matter, but I would like to point out that a fusion system is more than a plasma. We have the energy conversion system.

If we are going to have really an attractive product, research is needed—basic research is needed there. There is basic research needed in materials.

The issue of basic research is broader than what is brought out thus far if we really have an energy program, which is what—I would support Dr. Perkins' view in that respect.

Chairman ROHRABACHER. And, Dr. Conn.

Dr. DRUMMOND. Mr. Chairman, may I make a further comment?

Chairman ROHRABACHER. Yes, and then Dr. Conn.

Dr. DRUMMOND. I would like to say that I don't disagree that the notion of the engineering and developmental aspects associated with fusion are absolutely necessary. We are never going to have fusion until we learn about the materials problems and the control problems and the other problems. They are absolutely necessary.

But, we have at the present time a certain size program. And, we have to have priorities.

Now, it turns out that for the science part of that program, we have invested billions of dollars in machines which, in fact, are extremely fine scientific machines. We have no such investment in developmental apparatus.

And, if we are going to make the best use, the most effective use, of our funds and our scientific teams, we should proceed with the scientific programs since those are the assets we have to use. It's just a matter of priority, which comes first.

They all have to be there eventually, sir.

Chairman ROHRABACHER. Thank you, Dr. Drummond. Dr. Conn.

Dr. CONN. My comment was going to be that I guess I think we have got it just about right. What you can see is that there are things for which people said as, for example, on the basic plasma science as part of the program, they felt we had recommended exactly what they felt was appropriate.

In other areas, I think people are saying. Do we have a proper vision? I think we really do.

And, I can have a long debate with my friend, George Miley. What I would rather do is say the following. First, with respect to my colleague, Cliff Surko, basic plasma science as being defined by the National Research Council, I'm going to define as being not high-temperature plasma physics.

You know, the fluorescent light bulbs in this room are plasma. We haven't studied those very much in the fusion program, because they are not high temperature plasma physics, high temperature and high density.

There's a great deal that we learn from the studies of cold plasmas. My colleagues at UC, San Diego have pioneered the area of what's called non-neutral plasmas. They are very cold.

But, we learn enormously interesting and wonderful things by those studies. And, I think it's that sort of thing which the National Research Council had in mind.

That's why 5 percent ultimately makes sense. That doesn't mean that the rest of it isn't doing very good basic science, but it's doing it in high temperature plasma physics.

When you do that, I come to agreement with my friend, Dr. Drummond. In order to do the high-temperature plasma physics, you need the vessel to do it in.

You can't do that in very small, low-power experiments even at universities like mine. Some of those things have to be done with very high-powered devices.

And, that's why I think he's enthusiastic about machines like TFTR, DIII-D and C-Mod.

Finally, there has been one piece that no one has discussed. And, we discussed it, of course, in our report. And, I think it's the difference between the budget that Dr. Drummond remarked about and what we recommend to you.

And, that's the international program. The role of the U.S. program is very much now to leverage on that $1 billion worth of plasma and fusion energy research going on elsewhere in the world.

One vehicle by which we do that is our participation in the ITER program, which Mr. Roemer remarked about. We've made a commitment to be a part of that program, at least, through the end of the engineering design activity phase.

What decision we should make after that is something we haven't actually addressed. But, participating there is, in fact, bringing a great deal of leverage to the U.S. program.

And, it is a window, a current window, on the international activities. So, if you combine the programs that Dr. Drummond spoke about with what we recommend in the FEAC report and you add a significant element of making certain we are able to learn from what is happening internationally, you come to the program that I think you see us recommending.

As for product, we have put a lot of emphasis—and let we remark that there should be work on things like developing low-activation materials. You remarked about Dr. Lidsky's comment that they are very radioactive.

Fusion machines are kind of like us. They are what they are made of.

If you make them of materials that do get highly activated in the fusion environment, you will, indeed, get the outcome he spoke of. But, we've learned a lot over the last 10 or 15 years in, I would say, working with utilities.

And, we now know what materials will, in fact, produce much, much better vehicles for fusion power machines. And, I can go into that more.

And, we urge, therefore, that work on that continue. That's not basic plasma science, but it's basic fusion.

Chairman ROHRABACHER. Dr. Conn, your—and this goes, I guess, consistent with the discussion as it's emerging here, you basically are saying that the program—the report is saying that the program shouldn't be driven by development of a fusion power plant at a date certain. Yet—and what you just said and what I take it from the report and what's going on here is that we still have three tokamaks planned plus the ITER tokamak. And, you just talked about the ITER tokamak.

And, I think it was Dr. Miley who was suggesting that alternative programs may be being frozen out. Perhaps it wasn't you who made that point, but I wrote that down there.

Are we still moving forward? It seems to me that even though the report is heralding, you know, a change in concept that perhaps we don't have a change in concept here and that alternative programs and smaller programs and maybe some of the basic research is going to be frozen out by, you know, proposals that we still have—three tokamaks and the ITER tokamak.

Dr. CONN. Well, that's a good question. And, thank you for giving me the chance to address it.

The way we addressed it was as follows. We called for there to be a date certain in the program. And, that date certain was the termination after extracting what we felt was important, the remaining scientific value that I think Bill Drummond has been pretty eloquent in defending, TFTR.

But, we made a set of priorities. We laid out those three tokamaks. And, we said we thought that there was probably about a year-and-a-half worth of very strong scientific work that was remaining in TFTR and then it would have to be turned off.

And, part of that conversion would be to provide resources to foster alternative concept research and research on other ideas. So, there's a very systematic plan of, on the one hand, managing the program to extract the science that's really there to be gotten and moving the program in a new direction.

I must say that I don't believe in quotas, the notion that you are going to decide ahead of time 30 percent of a program is worth supporting or 25. When it's low enough like 5 percent—and I understand it very well—I can support that. And, we did.

But, there's no basis scientifically for a number like 25 or 30 percent for alternatives or any activity. What we said was there should be excellence and there should be review.

And, we should be determining by the quality of the ideas what it is that gets pursued.

Chairman ROHRABACHER. I was a bit concerned, just like last year when we were trying to put this budget together, that smaller projects that maybe aren't as grandiose and don't have as much public attention, things like—and I know this—I'm sorry, I'm not a scientist and I'm sorry if I'm saying something that sounds stupid to you folks who, you know, have studied this field a lot more than I have, but I've read a little bit about sonoluminescence. And, what appealed to me is that we could make a determination whether or not there was some type of fusion going on here.

With a very small amount of money, we could actually make that determination. And, either we could then move on or we could set it aside after a reasonable amount of money had been spent.

But, at the end of the budget process, thinking that was not going to be funded. Instead, we are going to have the money going into all of these huge programs while these other smaller ideas that might have some validity and could be proven at a reasonable expense would just not even be looked at.

Dr. CONN. Well, that's why we said at $250 million we would not continue to operate TFTR past about mid-1997. The purpose was to generate the money within the budget to do just what you are talking about.

That's work by my good colleague, Putterman, at UCLA. So, we are saying that there should be a balance of facilities that should be a means by which we determine which gets funded.

And, we have called for a set of priority orderings in any new facilities that are being brought along in the program to be small-to-medium scale. We are very explicit in saying at this budget level one would not entertain the notion of building a large, let us say, billion dollar class experiment.

And, so we are calling very much for there to be a larger number of medium- and small-scale experiments. We are making it clear, however, that there are good things to be had out of some of the experiments we have invested in.

And, we've tried to give you a date certain, if you like, how much longer to run what facilities in order to gain that scientific knowledge. I think that's a very responsible way to go at this rather than setting a quota and starting on that path right away.

Chairman ROHRABACHER. Okay, Dr. Conn. Thank you. And, Professor Drummond would like to say something now, but then I'm going to let George Brown, former Chairman Brown, proceed with some questions.

Dr. DRUMMOND. I would like to say something in support of what Bob just said. The important thing that is happening here is that the basis and the perspective on the program is going to be a scientific one as opposed to a developmental one.

And, then, given that perspective, a continuing evaluation of the scientific value of programs such as TFTR and the time to turn it on or off, or other programs, is a perfectly rational way to run the program as long as we understand that our perspective is on scientific merit as opposed to some developmental date certain fusion plant. So, I think what he has just said is absolutely right.

Chairman ROHRABACHER. Okay. Well, thank you very much, Dr. Drummond.

Chairman Brown, would you like to proceed?

Mr. BROWN. Thank you very much. I have been very pleased with this panel this morning.

I think that if we could entrust this panel with the continuing responsibility to monitor fusion collectively and report about every five years on what the next five years should be, we would be in good shape. Actually, that's the way the program ought to proceed, with a periodic reevaluation.

And, it has. But, I don't think it has been quite that consistent.

There are a number of programs that have this built into them as a process. For example, the National Science Foundation's Engineering Research Centers are looked at generally every five years and renewed.

NASA's Technology Centers are looked at every five years and renewed. And, we might need to do that here.

It is not the budget crunch that really compels this current reexamination. It's either a fortuitous or non-fortuitous coincident between the budget crunch and the end of the engineering design phase and the prospect of going on to the construction of a prototype machine that forces us to look at the fusion program.

And, I think most of you gentlemen would probably concur that we shouldn't go on to that prototype machine with the current state of our basic knowledge about the processes underlying that. Am I correct in that?

Or do some of you think that we ought to immediately go ahead? Dr. Conn?

Dr. CONN. What I would say is that in the current climate, it would depend on what they were asking our investment to be. If the Europeans and the Japanese, for example, either independently or collectively, decide, for their own reasons—and they have a very different energy situation than we do—that they want to go forward to build a machine such as ITER, which would then be the first machine that would actually do burning plasma physics, ignition, and do a good deal of the technology, and what they asked us to do would be to continue the level of contribution that we are approximately making during the current phase, I would argue to you, Mr. Brown, that that would be a very good investment for us to make.

If they were to ask us to pay 25 or 30 percent of the cost, I might reconsider.

Mr. BROWN. Yeah.

Dr. CONN. So, it depends on what it is that we are being asked and how our participation in the program is requested. And, I think it might, under certain circumstances, be a very wise investment for us to make.

Mr. BROWN. But, let me just ask you this—any of you can respond. Do you see any realistic possibility that our three partners are going to call for us to do that?

Dr. CONN. What I know at the moment is that we have heard that the Japanese themselves feel that this is very important for them, not only for energy but also because on reflection they realize they have never undertaken the leadership role in a major scientific enterprise.

Mr. BROWN. Yeah.

Dr. CONN. And, here is an opportunity for them to do so. If they step up to the plate and are willing to do that and provide funding at 80 to 90 percent of the total cost, then I think we would consider it.

What the Europeans will do, I think, is more problematic. I have no insights there.

Mr. BROWN. Other comments?

Dr. DRUMMOND. Sir, there are alternate ways to view the international cooperation. It may well be that in three or four years, based on the results that we get on our domestic science program, we will feel that we have achieved what we can achieve largely with at least one or perhaps two of the existing machines that we have.

And, if at that time we had an opportunity to enter into an international collaboration, not ITER but another international collaboration which was much more scientifically inclined, one in which we really had an interest scientifically and which we might be able to afford a smaller machine, to be a major partner, I think that's an alternative which we might find much more attractive.

Mr. BROWN. Yeah.

Dr. DRUMMOND. And, I think that should be considered as something else.

Mr. BROWN. Dr. Miley?

Dr. CALLEN. Jim Callen here, sir. Dr. Miley is over here.

Mr. BROWN. Okay.

Dr. CALLEN. Just to respond to your direct question about do our international partners perhaps welcome some other level of commitment, we, during the course of the various subcommittees meeting that Dr. Knotek particularly put together, met with a number of the ITER representatives. We were, frankly, pleased that they were able to accommodate what amounted to a 25-percent decrease in our level of contribution this year because of the budget constraints and that they were wanting us to continue to participate and seemed accommodative at various levels yet to be worked out, exactly not clear.

We would prefer obviously that as we move forward that we get into the more scientifically oriented areas, we could imagine then being involved in diagnostics, instrumentation and controls, perhaps a remote control site here in the United States. So, it seems to be some possibilities there.

Mr. BROWN. All right. Now, I don't want to belabor this and I don't want to exceed my five minutes.

So, let me proceed rather quickly. The figure that you are talking about of $275 million, under the scenario that you have, as a reasonable figure.

But, under a different scenario, say, a request for additional international participation with Japan carrying a major load, we might want to do more. Or, if there were some other interesting breakthroughs that demanded some expenditures in some of the more non-orthodox, we might want to demand more.

It's my view personally that we should move to a slightly higher level than this. But, I won't belabor the point.

We haven't looked at the impact of this on, we will say, the entrance of new students into the fusion field. They see a declining budget as an indication of no more career opportunities and the brightest people look for other things to go into.

Did you weigh all of that in recommending this figure? Just give me a yes or a no.

Dr. CALLEN. Yes. That's a large part of the interest in embellishing—or enhancing the plasma science part that Cliff Surko has talked about.

Mr. BROWN. All right. Now, let me just make one final comment, if I may.

This discussion has been very illuminating with regard to the perennial discussion we have of the difference between basic and applied research. You have illustrated that you cannot really make a firm division here.

And, those who say the federal government ought to support basic but not applied have to come up with a much better prescription than what we have at the present time. Originally, both cancer research and fusion research were considered basic.

Everyone knew that the end point was applied, that it was an energy source or a curative disease. It so happens that in cancer, the many different routes which also exist in fusion, each of those could result in the solution to a small part of the cancer problem. Fusion doesn't seem to produce that same sort of thing.

So, a lot of the cancer research is focused on a particular cancer and a short term possibility of getting results. And, although it's good basic research, it has closer applied connotations.

And, most of us in Congress don't make these kinds of distinctions. And, I'm urging my colleagues to pay careful attention to the descriptions that you have cited with regard to these nuances of the basic versus applied situations.

Thank you, Mr. Chairman.

Chairman ROHRABACHER. I think the former Chairman's insight that he just gave us is right on target. And, it is not as easy as some would suggest to determine what is the difference between applied and basic research.

And, so I accept that challenge. And, I will be looking at that much more closely.

Mr. BROWN. Thank you very much.

Chairman ROHRABACHER. Thank you very much, Mr. Chairman.

Mr. BROWN. You are a very broad-minded person.

[Laughter.]

Chairman ROHRABACHER. I would like to now turn to Mr. Roemer. Mr. Roemer, would you like to proceed?

Mr. ROEMER. Thank you, Mr. Chairman. Let me follow up on a couple of questions that the distinguished former Chairman, Mr. Brown, has asked of the panel.

Certainly, with more members here than were here with the opening statements that the Chairman of the Subcommittee and I made, we have gone from $366 million to $244 million in terms of funding in this particular program, about a third cut. And, we have heard, I think with unanimity, that $275 million is almost a bottom line for maintaining a viable R&D fusion program.

Even Professor Drummond, who was mentioning $212 million, was discussing that just for the domestic side. And, if you add in the $55 million for ITER, you get up to about the $275.

Dr. DRUMMOND. It's $8 million for Germantown.

Mr. ROEMER. Right. So, you are in agreement of that basic level. So that there is a consensus here.

We will hear different figures from the next panel. So, I want to make sure that there is no confusion on that point.

I do want to ask Dr. Conn and maybe Dr. Miley a question about the President's Council of Advisors on Science and Technology released a report saying that $325 million or $320 million was the basic level for a world leadership role for the United States in this area. Can you try to succinctly tell me what is the difference between $320 million and a leading role and $275 million and sustaining a viable domestic program with some kind of international component with ITER?

Dr. CONN. I think it's basically how you would participate in the major next scientific frontier step for this business that we haven't discussed this morning. And, that's burning fusion plasma, true ignition of a plasma with fusion fuel in it.

And, that's why I remarked about the ITER program having that as one of its objectives. In the PCAST report where they recommended a budget of $320 million, they recommended a significant commitment to a devise aimed at getting at the fundamental science of burning plasmas.

And, it asked that the United States go ahead and enter discussions with our partners about whether they would consider a smaller scale machine specifically for that targeted purpose. And, our estimate was that that would be a $4 billion or $5 billion machine as perhaps 40 percent of the cost of the ITER.

And, we were considering that we would then be approximately an equal partner in that. An equal partner would have been a billion dollars over 10 years or about $100 million a year.

That means you would double the $55 million we currently have. So, you add that to the $275, you come closer to the $320.

But, now you are playing a more substantial role in that international program. And, you are focusing on a very big hit issue for the science in the program.

And, we were asking, even at that time, our international partners to look at how far they wanted to go on the development path with the next step. So, that, I think, is basically the difference between what PCAST was recommending and what we are now recommending.

And, we were very clear that with the budget that we have now we will not be exercising the kind of leadership we could have at a somewhat higher level.

Mr. ROEMER. Dr. Miley?

Dr. MILEY. I see it slightly differently. That is, if the current experiments are maintained, there isn't room in the budget to obtain the intellectual leadership in the basic science and in alternate concepts, which I believe are a road to the future vision I was talking about.

And, so I don't know if that small amount of money will give us world leadership in the—I'm not sure in terms of a big experiment or something. But, it will give us a base program which will excite students.

I think you have to have more than money to excite students. You have to have a vital research program that is going to be exciting to them and lead somewhere. You will have that.

Plus, you can begin networking with international experiments throughout the world that are aimed at this search for the alternate.

Mr. ROEMER. Let me get to my second question, because I don't want to run over on time. And, I want to follow up on Mr. Brown's question that Dr. Conn answered on ITER.

Certainly, there is consensus here on this panel that we should maintain our commitment to ITER through the design phase, through 1998. That's correct.

Secondly—okay, Professor Drummond would disagree with that.

The second question would be. What about our——

Dr. DRUMMOND. Sir, I don't want to be in disagreement. I simply say that's beyond my kin. And, I think it's beyond all of our kin, because it's an international relations problem.

Mr. ROEMER. Well, okay.

[Laughter.]

Mr. ROEMER. Secondly, about the construction and the potential of the ITER project, do you advocate U.S. participation in ITER after 1998?

We heard Dr. Conn saying it depends upon what kind of participation we are asked to make in terms of science and money. Would anybody say, from this panel, no, we should not proceed based upon what we know now?

Let me get Dr. Knotek.

Dr. KNOTEK. Well, we spent a great deal of time in our study talking to the members of our fusion community. And, there are significant issues outstanding about the ITER design.

And, what we recommend very strongly is that we have the most rigorous possible review of that design. And, our participation should be based on the merits of that review and the merits of the design.

The Europeans are doing exactly the same thing. They have a very extensive review starting this summer by a committee that is totally outside of the fusion community to look at that design and determine whether it's feasible and to look at all the positives and negatives of that particular design at this time.

And, I think that's the only rational way to proceed, is on the merits of the design when it's ready to go.

Mr. ROEMER. So, you are saying, assuming the Europeans are going to automatically stay in this is not a safe assumption?

Dr. KNOTEK. I think all the parties are going to take a hard look at this design and look at the cost and the technical challenges and risks. And, they are going to make a determination based on that.

And, I don't think we should make a determination before that happens. I don't think anyone is.

I think there are many issues that are now being studied. There is a very open and rigorous process for looking at the science and technical challenges of that machine.

It's going on. And, we shouldn't make a prejudgment about whether it's going to be successful.

Dr. CONN. If I could just add one point. That is, in fact, the argument for why we should remain a strong participant during this coming period, so that we would then have the benefit of all of those reviews and we would make a determination——

Mr. ROEMER. Up to 1998?

Dr. CONN. Right. And, then we would make a determination about the appropriate level of participation, including not participating.

Mr. ROEMER. Dr. Perkins.

Dr. PERKINS. Sir, I was a member of the original design team for ITER in its first phase of operation, the conceptual design activities of about six years ago. Let's not kid ourselves with each other.

We know today, without another in-depth review, what we are getting into here if we go with the current machine. The current machine is very big.

It's very complex. And, it's not a great advertisement for a future fusion power plant.

That's known today. I don't think any review in the next two or three years are going to change those views of almost anyone in the program.

And, so I would, I think, strongly support Dr. Conn's view that we should probably not do this if we are asked to be an equal partner, because it will suck up every bit and more of our available fusion budget. If, however, it's a leveraged investment of about $50 million a year and let one other country or countries take it and shoulder that real visibility problem of building a power plant that everyone in the world is going to look at and say, "What is this thing," then I think it's a good investment.

Could I also just very quickly add one question? International collaboration of fusion is very important.

The question is. Is ITER a machine or is it an international process to do fusion? It doesn't necessarily have to be a single machine.

Thank you.

Mr. ROEMER. Thank you.

Chairman ROHRABACHER. Thank you, Mr. Roemer. I am going to move on now to Ms. Johnson from Texas.

But, I would like to ask you folks on the panel to be thinking about this as we go through, and at the very end I'm going to ask you to give me a one sentence answer. If, indeed, the fusion program is worthy of higher commitment, a higher commitment of dollars on the part of this Committee, where would you have us cut in science and research?

Where do you think those dollars are not being spent i a way that is not as important as your own program?

[Laughter.]

Chairman ROHRABACHER. And, I will say that every time I seem to ask this question and I try to get people to give me their highest priority and their lowest priority, they only give us the highest priority. And, they are never willing—if you can give me a one sentence answer to that at the end of this hearing of where you want it to be cut, that will be very much appreciated.

Mr. ROEMER. Let me make a suggestion. You might look over there and just say two words—the Space Station.

[Laughter.]

Chairman ROHRABACHER. I knew he would muddy the waters some way.

Mr. VOLKMER. I could give you another one—Star Wars.

Chairman ROHRABACHER. All right. Now, you've heard from the politicians.

We want to hear from the scientific geniuses here before us today. Okay.

With that, Ms. Johnson.

Ms. JOHNSON. Thank you very much, Mr. Chairman. Thank you for calling this hearing. And, let me express my appreciation to the panel.

As I have listened to some of the comments, I feel the pain of having been very interested and supportive of the Super Collider and its research. And, my question, I guess I would like to ask Dr. Drummond first, is very similar to the one our Chairman just mentioned.

In view of what we know to be frugal dollars for research for the future, how do you have—what is the vision of our fusion research in this country and whether it is the wise direction to go to be supportive of the international partner, as the international partner?

Dr. DRUMMOND. Well, it appears to me that from the budgetary point of view, the current level of science research that we had in 1995, which was around $212 million or something like that, in terms of the science research, I think that's a sustainable level. And, I see no reason why there should be significant increases in the need for that for the next several years.

To guess beyond that would be guessing about what kind of results we were going to get. And, I simply am unable to do that.

But, I see no need for escalation of our basic science program beyond that which we are capable of today, which is about that figure.

Whether or not we enter into international programs is a thing I think we should decide at the time, whether the programs are consistent with and supplement our domestic programs. And, remember, our domestic program is our central program.

And, we are not going to get free fusion just by being a partner abroad. We have to collaborate in programs which, I believe, we draw also scientific knowledge we want.

So, I would not think of this program as one which is escalating in the next four or five years in any significant way. But, I think consistency of the program is very important.

And, I think it is extremely important that the Congress impose a discipline on this program, that it actually have a scientific perspective. And, that takes a very different perspective than we have had under what was the National Energy Act, which told us to do something else.

We need now for our management to be scientifically very, very aware.

Ms. JOHNSON. Thank you. Thank you, Mr. Chairman.

[The complete opening statement of Ms. Johnson appears on page 153 of the Appendix.]

Chairman ROHRABACHER. All right. Mr. Volkmer, from Missouri.

Mr. VOLKMER. Thank you, Mr. Chairman. I would like to, first, ask very briefly for a brief answer, if I can, from anybody that wants to respond.

We have been on this road for approximately 50 years. Do we know now more than we knew 50 years ago?

Dr. CONN. Well, I will start with that, if you don't mind. We know an enormous amount more than we knew 50 years ago, both about the science of this business, about how to try to move forward towards making it into a form that might lead to practical energy, and we know a tremendous amount more about this field that enables a lot of spin-offs which people don't often think about.

I had been involved in the start up of a company, a very entrepreneurial thing to do in the United States. And, that company is based upon a set of ideas that came out of plasma physics.

And, it is now manufacturing the cutting-edge machinery for the manufacture of computer chips. And, it sells it not only to Intel and Motorola but, frankly, it's a worldwide company that sells it to Hyundi and Cannon and NEC and so on.

The derivative—the place where that came from was the science of plasma physics. We understood how to design a better plasma to etch chips.

And, that has created jobs in the United States. It has created leading edge technology.

So, we not only have, I think, moved enormously far forward in knowing how to move towards fusion science, but we've had a big impact economically on our own economy.

Mr. VOLKMER. Does anybody else want to comment briefly? Yes.

Dr. CALLEN. Just somewhat along the same line. Basically, the entire fusion budget has long fostered most of the entire development of the—or initiated the plasma physics. It has had many elements of offshoots, many applications—ion implantation, plasma stretching—and, as Professor Conn mentioned, is having a big impact of new developments in semi-conductor directions.

And, as emphasized by Dr. Surko earlier, it has been over the last few years that there has been a narrowing of the program which has been decreasing that because of an energy development initiative or direction, as Dr. Drummond mentioned. And, it's to reassert that role and keep that very viable, which is a very important part for many areas of science and technology in the country.

Mr. VOLKMER. All right. Now, we still have a need for funds. I believe this is why we need to go back to the university looking at theory in the areas.

Is that correct? Is that the purpose of it, a different theory? Or, is it using the same theories we have already developed?

Dr. SURKO. Oh, no. Well, the Research Council report recommends that the crying need is for small-scale experiment, but also it will have strong connections to theory.

And, in response to your first question, there has been tremendous advances in our ability to create and manipulate hot plasmas and in the theory of those plasmas. But, it's—a theory without seeing whether it works in the physical world doesn't go very far.

Mr. VOLKMER. That was my next step.

Dr. SURKO. So, it's a close connection of a small-scale experiment, large experiments and theory.

Mr. VOLKMER. All right. That was my next—all right, Dr. Drummond, first.

Dr. DRUMMOND. I think the point is that theorists are cheap to keep, so they don't get mentioned in budget discussions.

Mr. VOLKMER. All right. Without a methodology to demonstrate your theory, the theory isn't worth very much, is it?

Do you agree with that?

Dr. SURKO. That's right.

Mr. VOLKMER. Now, we have the next panel coming up, and they say we should cut off the TFTR next year—that's gone; that General Atomics should go out and within three years be able to fully fund their DIII-D; and, that we shouldn't fund MIT for any longer than five years.

Now, tell me, what does that do to proving any theory that calls for a large experiment? Tell me what that does.

Dr. CALLEN. Well, obviously it cuts off investments that we have, as Dr. Drummond has mentioned earlier. We have made tremendous investments, the Congress has, in the field of people, scientific experts——

Mr. VOLKMER. The taxpayers have.

Dr. CALLEN. Right, to assets, which are there, are working, are tremendously active, are doing extremely good science. And, it would be a real pity to shut them off when they are at so high performance level.

Mr. VOLKMER. You know, I would be interested—we don't have anybody from General Atomics here on the panel. I would be interested to find out from them—and perhaps, Mr. Chairman, we can inquire by letter—as to whether or not they feel they are going to be able to fund from private sources and continue the operation of their DIII-D.

Dr. DRUMMOND. It seems unlikely.

Mr. VOLKMER. I think it's going to be very unlikely.

Dr. KNOTEK. Could I make another comment on that?

Mr. VOLKMER. Yes.

Dr. KNOTEK. The complexity of restructuring this program is really one of how do you go from a program that was really driven by the Energy Policy Act of 1992 and which really drove this focus on tokamaks as the central technical thrust, because that was really the only one with that opportunity to get to the goal of the Energy Policy Act of 1992, to a restructured program with a much broader scientific and institutional base. And, the thing that is very important to do is to make full advantage of the investment this country has made in these tremendously versatile three major facilities in this country.

How we use them, how we extract knowledge of them and draw down the nation's investment in them is critical to the future. We cannot just switch to an alternatives program and leave behind well over a billion dollars in investment.

What we have tried to do in this program is allow more of the scientific community to use those facilities for a broader set of scientific goals that could then provide the basis for our invigorated next generation of experiments, which would have many more alternatives than the tokamak concept; and, also, as well, look at a generation of machines that are not only future power plants but are much more capable of providing insights into the scientific underpinnings of the technology.

Mr. VOLKMER. Well, to me, it would be a huge waste, at least of the knowledge that is capable of being developed if we follow this procedure.

Dr. MILEY. You are throwing——

Chairman ROHRABACHER. Thank you very much.

Mr. VOLKMER. Dr. Miley would like to answer.

Chairman ROHRABACHER. Go ahead, Dr. Miley. Then, we will move on to our next questions.

Dr. MILEY. Well, I certainly am against this waste. I am against this waste but, on the other hand, impatient and a little bit worried.

In the past, the first thing you would cut out of the program was alternatives. And, it seems to me that what we have to do is ensure that it's more than a promise that they are going to come back three years hence or something, that they have to start coming back now and some way the re-engineering has to include that element.

Chairman ROHRABACHER. Thank you very much, Dr. Miley. There is no doubt that there is not going to be any change unless the government starts making decisions to set limits on how much money will be spent, however.

If we keep shoveling money out of the back of the truck, nobody is going to try to be more productive. That's the bottom line of it.

And, we will never ever get the private sector involved if the government is willing to pick up everything, because all the scientific community will gravitate right towards that guaranteed source of income. What we want to make sure in this country is that we get the most efficient use out of every dollar we spend on science and research.

And, sometimes that doesn't mean spending more and more money. And, with that, I will turn to Mr. Bartlett, who is one of our few scientists who actually has been elected to Congress.

And, we appreciate his expertise and his presence with us today.

Mr. BARTLETT. Thank you very much. I would first like to identify myself with some of the remarks that George Brown made.

I appreciate very much what you represent. And, I agree that you, as the panel, probably are a whole lot more competent to decide the future of this program than we, who must decide that future, are. And, we appreciate very much your input.

Now, I can still remember the first time that I took a drop of water from a pond and put it under a microscope. And, I remember my feelings when I was exposed to this whole new world that I never knew was there of rotifers, euglena and paramecia and amoeba and so forth. And, I think that the world inside the atom is even more exciting.

But, somehow, as a society, we have failed to convey this excitement to our people. The only time we came close to conveying the excitement of pushing the envelope was when we put a man on the moon. And, we identify, each one of us in the country, with that activity.

I voted to continue the Superconducting Super Collider, because I felt that there may be observations there that could focus the imagination of our people, capture the imagination of our people and excite our young people so that the bright minds in this country would again want to go into science, math and engineering. And, I bring this up because it is hurting us.

And, it's not your fault. It's the fault of our society. And, we collectively have to address it.

Our inability to convey the excitement of what you are doing to the community at large does two things. One, it limits the amount of dollars that are available for these good programs, because we are, in large measure, a reflection of the people who sent us here. And, that's what we ought to be, I guess.

But, it also really limits the number and quality of our young people who go into these areas. And, our future here is going to be determined by that.

Yes, sir, you would like to make a comment?

Dr. CALLEN. Thank you. I would just like to very much echo your comment and say in the fusion program there are a bunch of very energetic, young, intellectually interesting people who find fusion a very stimulating thing they would love to work on. And, the budget cuts of the last year and the uncertainty—if we can just get a little stability here, I think it will bring back those people.

And, it's a way of attracting people into sciences, because people really get into the long term energy need interest.

Mr. BARTLETT. What you say is very important, because as we cut these programs and we limit opportunities for our bright, young people to go into these areas, fewer and fewer of them are going to go into it. And, too few are going in now.

We could spend a long time on this. But, I feel a real need to focus the imagination of our people on these exciting things that we are doing.

And, we don't do a very good job of that at all. It's all ho-hum now.

We have a launch and do really spectacular things in space. And, our people are watching soap operas instead of being excited by this.

Dr. Perkins, you made a comment that I know there was something in your thinking when you said this. We can't explore it very far in the five minutes that we have here, but to me it's one of the most significant underlying problems that we face.

You made the observation that fusion was the only indigenous energy source that will last as long as the earth lasts. And, I know that there was a lot of background information in your thinking.

Could you spend just a moment or two and tell us why you said that?

Dr. PERKINS. Yes, sir. About roughly one in 10,000 hydrogen atoms in the sea water, which covers the majority of the plant, is, in fact, not a hydrogen atom but a deuterium atom. So, roughly the isotopic ratio of deuterium to normal hydrogen is one in 10,000.

If we were to burn that with the DD, deuterium/deuterium fusion reaction, then there is enough energy there to last humanity forever. Pure and simple.

Mr. BARTLETT. Why do we need that energy? Isn't there enough coal and oil and so forth to last forever?

Dr. PERKINS. That's a good question, sir. In the—there are actually only three long term potential energy sources for global base load electricity generation, in my opinion.

The fossil fuel such as gas is short term. Coal may last about another 300 years or so but with possibly disastrous environmental consequences in the next century.

And, so those three are advanced fission, possibly solar electric and fusion. And, that's about the only three we can see that will last beyond about the next 300 years or so. .

There may be others we have not yet discovered. But, that's all we know about at the moment.

Mr. BARTLETT. Okay. Okay, thank you very much.

Chairman ROHRABACHER. I would like to thank Mr. Bartlett. We will have one or two questions from Mr. Baker from California.

And, then the Committee would just let you know what we are going to be doing. We will break.

There is a vote going on. We will break for about 15 minutes or 20 minutes for this vote.

And, so if you want to pick up a sandwich or something like that in between, this might be the time. Then, we will immediately reconvene about 15 or 20 minutes later.

Mr. Baker?

Mr. BAKER. Chairman Rohrabacher, I would like to ask Mr. Perkins, in your testimony you mentioned alternatives and that inertial confinement fusion is particularly interesting. The question is important, because inertial fusion for energy, despite its promise and despite numerous favorable scientific reviews, has been treated as a stepchild in energy research.

What are your thoughts on this?

Dr. PERKINS. That's a very good question, sir. Inertial fusion energy has two very important features.

The first is that it's thoroughly grounded, I think, in good science, both in the U.S. and the world. And, secondly, it's a fundamentally different way of doing fusion compared with the tokamak or devices like the tokamak; I mean, fundamentally different, different in its physics and in much of its engineering.

I also, I think, second your view or your implied view. I think inertial fusion energy is getting a raw deal in the current fusion budget.

I would suggest the Office of Fusion Energy is charged with the stewardship of the Fusion Energy Program. And, inertial fusion energy from inertial confinement fusion should be a part of that.

And, it certainly is deserving of funding at a higher level than it gets at the moment from within the conventional fusion budget. Let me also just add that it leverages considerable investment off the defense-related activities in the United States, such as the National Ignition Facility and devices of that.

Mr. BAKER. So, it's complementary.

Dr. PERKINS. Absolutely.

Mr. BAKER. But, if I could just—because we are going to have to run. Does everyone think that it's realistic?

Is this science that we ought to be pursuing, the inertial confinement fusion?

Dr. CONN. May I add a comment about the purpose of its being pursued? When we talk about basic science, I think the purpose of the main program in inertial confinement fusion, which is funded by the defense side of the DOE, is at the moment for stockpile stewardship.

And, as best I can tell from reviews I have served on and from what I've read, that is a very important mission. And, I don't have any problem there.

The issue that has occurred, when you remarked about the stepchild nature in energy research, has been that there is a program on heavy ion, the development of heavy-ion accelerators, to produce heavy-ion beams which might be then the driver to make targets implode. And, that program used to be in Basic Energy Sciences. And, it was an orphan.

The Defense Programs felt they didn't need it for the emission. And, so the program eventually in 1991 or 1992 got put into the Energy Research Program.

But, it's not magnetic confinement approach, which has been—it was the Magnetic Fusion Energy Program. And, so the problem has been, in fact, that this is an area of research which has received good reviews every time it has been reviewed. You are absolutely right, the heavy-ion beam part of the program.

But, for one reason or another, it has not found what I would call a supported home. The best home would be in relationship to the Defense Programs, because they are the ones doing all the research on the targets for fusion.

And, this is a driver to drive those targets. They ought to be in direct contact. And, they ought to be in direct interaction with that community.

And, so the orphan problem is that they are placed in a program which has mainly been studying magnetic confinement of plasmas. It's not that we don't like it or people in that program don't like it or that it's not a very good program.

The orphan nature is, therefore, related to that. Where it best fits is where the people are really doing the work that it's being developed for. And, that's DP.

Mr. BAKER. Anyone else? Sir.

Dr. MILEY. I would certainly second those comments. But, if we really take inertial confinement fusion seriously as a power source, we need to broaden the research base again there, because there are many issues that would have to be included to study a complete system.

And, these are grossly under-funded. In other words, only heavy-ion beam fusion, one driver—one approach to a driver is being funded. We need to fund alternate driver concepts.

We need to look at reactor relevant research—liquid—metal protected walls. All these things are very complex and need to be studied if we take it seriously.

I think this may be very good if you think about the analogy with the development of fission power, which came about really in partnership with the nuclear submarines. If the nuclear submarine hadn't maintained a level budget fission power it would not have been developed in the time scale it was.

Perhaps that's the only way that this can proceed and then——

Mr. BAKER. Well, maybe the ignition facility will give us the boost. But, it does have an energy component.

Dr. MILEY. Yes, sir.

Mr. BAKER. And, I just didn't—I wanted to make sure no one was going to tell me we are barking up the wrong tree.

Dr. DRUMMOND. Mr. Chairman, may I be excused? I have a plane to catch.

Chairman ROHRABACHER. You certainly may. And, we appreciate very much you being with us today and sharing your expertise with us.

I would like to thank the panel. We will—now, let me ask if there is anyone who wants to return. Mr. Ehlers, are you going to want to ask this panel some questions?

Mr. EHLERS. In the interest of time and because we have to vote and to release them from any obligation to stay, I will just make a very brief comment and forego any questions. And, that is to thank the panel for their work.

As a scientist, it's a pleasure for me to see you work together and try to come to some joint agreement on priorities for our spending decisions. As you can imagine, it's very difficult sitting on this side of the table.

My colleagues asked Dr. Bartlett and myself to make these decisions for the Congress. We are not the experts that you are in your field.

And, your report has been very helpful to me. And, I appreciate the work that you have put into it.

I obviously don't agree with everything in it. But, it is certainly a good guide for me and for the other Members of Congress.

And, I appreciate the efforts you have put into it. Thank you very much.

Chairman ROHRABACHER. Mr. Ehlers is another one of the scientists. I think—do we have three or do we have two?

We have three scientists. Mr. Olver was here as well. And, so we appreciate you today.

Because we seem to have gotten the questions out of the way, we can dismiss you as the first panel. So, you don't have to come back unless you want to hear the testimony of the second panel.

I do want to express my appreciation. Thank you very much.

And, I think we have raised some serious issues and got some dialogue going that will, as I say, resonate beyond just this one room. We will hear it throughout the United States.

So, thank you very much. This Subcommittee is in recess until 12 o'clock.

[Brief Recess]

Chairman ROHRABACHER. The Subcommittee will come to order. And, our second panel will address the policy recommendations of the Fusion Energy Advisory Committee.

Dr. Martha Krebs is Director of the Office of Energy Research at the Department of Energy. And, we welcome you, Dr. Krebs. And, we are very happy we were able to work this out.

And, Tom Schatz is President of the Citizens Against Government Waste, of course, a taxpayers watchdog group.

And, Joseph Gavin, a retired executive with Grumman Aerospace and cast a dissenting vote as a member of the Fusion Energy Advisory Committee, a man who has also been very generous in giving us in Congress your advice. And, we are very appreciative of the advice you've given us in areas of aerospace and areas of dealing with science and technology.

So, we are very happy to have you with us today. And, thank you very much for your contributions.

Mr. GAVIN. I would also appreciate making it clear that the sign has my name wrong. My middle initial is "G," and there is no "l" in my name.

Other than that, everything is good.

[Laughter.]

Chairman ROHRABACHER. There is a man who watches out for the details.

[Laughter.]

Chairman ROHRABACHER. Good for you. And, James Adams, who represents the Safe Energy Communication Council.

And, with that, I think we should start off with Dr. Krebs. And, if I could just recommend again that, if you would summarize, your full testimony will be submitted for the record.

If you could, summarize and go to your basic points, then we could promote a dialogue among the panel, which I found in the last panel to be quite a valuable exchange.

So, Dr. Krebs, if you would like to proceed.

STATEMENT OF DR. MARTHA A. KREBS, DIRECTOR, OFFICE OF ENERGY RESEARCH, U. S. DEPARTMENT OF ENERGY

Dr. KREBS. Mr. Chairman, Members of the Subcommittee, I am pleased to be here today to discuss the Department of Energy's perspective on the Fusion Energy Science Program. And, I do appreciate the efforts made by you and your staff to accommodate my schedule.

Before I go into my remarks, I do want to appreciate, from the point of view of the Department of Energy and the Office of Energy Research, the efforts of Dr. Conn, Dr. Knotek, Dr. Callen and other members of the Subcommittee—of the FEAC that produced the report. In less than two months, during a period of great turmoil, they produced a report which advocates significant changes in almost all aspects of the program but has nevertheless been widely accepted by the fusion community.

It was a demanding schedule. They met it. And, they had to deal with a lot of personal issues, both in terms of their own scheduling but also with the things that were happening inside the fusion community.

I would like to expand briefly on the effects of the steady decreases in fusion budgets since 1980, combined with an essentially unchanged energy technology development mission for the program. Since the energy crisis of the 1970s, fusion program plans have assumed out-year budgets that would at least double within five to 10 years in order to accommodate the facilities required by the technology demonstration mission.

In 1976, the Energy Research and Development Administration, ERDA, estimated that in order to construct a demonstration plant by 1998, we would have to have a program that peaked at $2 billion in 1996. In 1990, the Fusion Policy Advisory Committee recommended a program leading to a demonstration power plant in the year 2025 that required a program funding of slightly over $700 million in fiscal year 1996. And, it was on that basis that the Energy Policy Act recommendations on fusion were provided.

In 1995, the PCAST recommendations estimated that simply retaining core elements of the current program would require constant funding of $320 million with an expectation that if large facilities were built or participated in internationally that the requirements for that funding had to be built on top of the core program.

The ERDA estimates seemed feasible at the time. But, as priorities changed, the prospects for such funding increases faded.

Previous Administrations and Congresses postponed the projected increases and were reassured by the fusion community that this need not change the ultimate technical goals of the program. But, what did happen was that the scientific and technical scope of the program narrowed and affiliated technology research and facility planning and investment were stretched out over a long time frame.

The program was faced with insufficient resources to accomplish its energy technology mission. And, that made it vulnerable to charges of uncertain vision, poor management and lost scientific opportunity.

Action by this Congress in 1995 gave the program and this Administration an unambiguous message—there will be dramatically fewer dollars for the foreseeable future and the long-term focus of the program must change to research instead of technology demonstration. Nevertheless, the program and its researchers in this period made impressive scientific progress.

I'm not going to summarize that. But, I think it's in my testimony and I believe you've probably heard of it in the testimony of the earlier panel.

As it stands, the program has a strong base from which we can make the transition to the world class fusion energy science program. The first step in that transition was the FEAC report.

I fully endorse, and the Department has accepted, the program mission and goals proposed by FEAC. The Department's fiscal year 1997 budget submission will reflect a program restructured in accordance with these recommendations.

The elements of that program are. A plasma science initiative that will be a new start essentially in 1997, hopefully will grow in the out-years to about $10 million and will be coordinated with other agencies who rely on the development of plasma science, such as NASA and the National Science Foundation.

There will be an increased emphasis on alternative concepts. We will have a changed but continuing role for the Princeton Plasma Physics Laboratory.

The FEAC recommends the shut down of TFTR after exploiting its scientific capabilities but emphasizes the need to maintain the technical and human infrastructure provided through decades of investment at the laboratory. These are going to be the base of core competencies required for national leadership and international collaboration in fusion research.

We will shut down the TFTR in 1997 or 1998, depending upon the recommendations—of our scientific—and the progress of the scientific results that come from that facility.

There will be no large facility constructions. And, by large, I mean things on the scale of the Tokamak Physics Experiment, which was on the $700-million to $800-million level.

Resources will be concentrated on increasing our fusion science knowledge base and construction of a limited number of small- to medium-scale experiments funded within an essentially-flat budget.

We will leverage our program resources through international collaboration. We expect to include international collaboration.

Beyond the ITER project, there is already a significant amount of international collaboration but not as formalized as TFTR—excuse me, as ITER. And, we plan to be an active participant in the ITER EDA through its completion in July 1998.

Given the financial limitations, the Department will not seek to be the host of the ITER facility. But, international collaboration will continue to be integral to a restructured fusion energy sciences program.

I believe that this restructured program, Mr. Chairman, built upon the carefully thought out FEAC report and the input of fusion science—of the fusion science and technology community is responsive to the guidance provided by the Congress last year. It strengthens the fusion science and plasma science programs of the nation and broadens the knowledge base and the human resource base needed for an economically and environmentally attractive fusion energy source.

This concludes my presentation.

[The prepared statement of Dr. Krebs follows:]

STATEMENT OF

DR. MARTHA A. KREBS

DIRECTOR

OFFICE OF ENERGY RESEARCH

BEFORE THE

COMMITTEE ON SCIENCE

SUBCOMMITTEE ON ENERGY AND ENVIRONMENT

UNITED STATES HOUSE OF REPRESENTATIVES

MARCH 7, 1996

Mr. Chairman and Members of the Subcommittee:

I am pleased to be here today to discuss the Department of Energy's Fusion Energy Sciences Program, and the excellent recommendations of the Fusion Energy Advisory Committee (FEAC) presented in their report entitled, "A Restructured Fusion Energy Sciences Program," and the National Research Council's report, "Plasma Science," which will inform and guide us in restructuring that program.

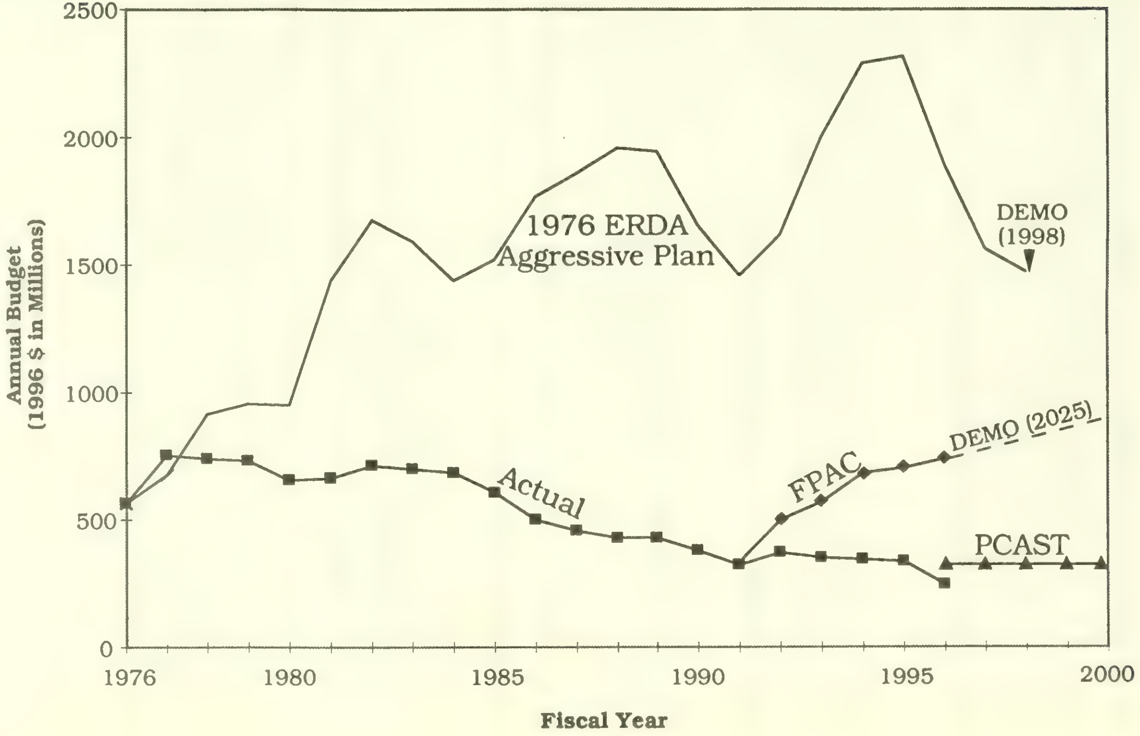
Before addressing these subjects, I would like to express my appreciation to Dr. Conn, Dr. Knotek, and Dr. Callen for their hard work and leadership during the production of the FEAC report. In less than two months, during a period of great turmoil, they produced a report which advocates significant changes in almost all aspects of the program, but has nevertheless been widely accepted by the fusion community. In spite of a demanding schedule, they were careful to reach out to include a broad cross section of that community. I commend them for including in their subcommittees younger fusion scientists, whose careers will be most impacted by the changed fusion program. Their report is a remarkable achievement.

The Department's fusion program is now at a turning point. It is changing from a goal-oriented energy technology development program to a fusion energy sciences program. In the charter for this hearing, there is an excellent summary of the history of the Department's program and the events which force this change. I will not repeat that history, but would like to expand briefly on the effects of steady decreases in fusion budgets since 1980, combined with an essentially unchanged energy technology development mission for the program.

Since the energy crisis of the 1970's, fusion program plans have assumed out-year budgets that would at least double within five to ten years in order to accommodate the facilities required by the technology demonstration mission. In 1976, the Energy Research and Development Administration (ERDA) estimated that, in order to construct a demonstration power plant by 1998, annual program funding would have to rise dramatically, peaking at well over $2 billion in 1996. In 1990, the Fusion Policy Advisory Committee (FPAC) recommended a program leading to a demonstration power plant in 2025, requiring program funding of slightly over $700 million in fiscal year 1996. In 1995, the President's Committee of Advisors on Science and Technology (PCAST) estimated that simply maintaining the core elements of an energy development program required constant funding of $320 million. The reality of our funding for this year, of course, is $244 million. The funding profile for each of these programs is illustrated in Figure 1, along with the actual program funding.

The ERDA estimates seemed feasible in the light of the energy crisis of the period, but as priorities changed, the prospects for such funding increases faded. Previous Administrations and Congresses postponed the projected increases, and were reassured by the fusion community that this need not change the ultimate technical goals of the program. But what did happen is

Fusion Program Budget History



that the scientific and technical scope of the program narrowed, and affiliated technology research, and facility planning were stretched out over a longer time frame. The program was faced with insufficient resources to accomplish its energy technology mission, and that made it vulnerable to charges of uncertain vision, poor management, and lost scientific opportunity.

Action by this Congress in 1995 gave the program, and this Administration, an unambiguous message--there will be dramatically fewer dollars for the foreseeable future, and the long-term focus of the program must change to research instead of technology demonstration.

Nevertheless, the program and its researchers have made impressive scientific progress. Since the mid-1970's, the performance of fusion experiments, measured in terms of fusion power output, has increased by a factor of 100 million. Last year, the Tokamak Fusion Test Reactor (TFTR) at the Princeton Plasma Physics Laboratory (PPPL) produced over 10 million watts of fusion power for about one second. More importantly, however, these experimental results have led to increased theoretical understanding, while sophisticated diagnostics and new computer control techniques have made it possible to understand and control complex plasma experiments. A combination of this theoretical understanding and experimental capability has recently led to prediction and demonstration of an important new way to operate tokamaks called "reverse shear" operation. This phenomenon, observed on both TFTR and the D-IIID tokamak in San Diego, opens the possibility of much more efficient energy production in a tokamak than previously thought possible.

In addition to this progress in fusion science, the program has served as an incubator for the increasingly important and interesting field of plasma science, and has also been a model for international planning and cooperation in sciences. As it stands, the program has a strong base from which we can make the transition to the world class fusion energy science program recommended by FEAC.

The first step in that transition was my December 5, 1995, charge to FEAC to restructure the fusion energy program in accordance with the guidance provided by the Congress in the Conference Report for the fiscal year 1996 Energy and Water Development Appropriations Bill. The FEAC report, "A Restructured Fusion Energy Sciences Program," offers the Department a thoughtful and balanced blueprint for the future of a fusion sciences program. It is responsive to both its charge, and, I believe, to the National Research Council recommendations regarding the Nation's need for a plasma science program.

I fully endorse, and the Department has accepted, the program mission and goals proposed by FEAC. The Department's fiscal year 1997 budget submission will reflect a program restructured in accordance with FEAC's recommendations.

The first of the changes in the program is its name--the Fusion Energy Sciences Program. The substance of the program will change significantly in response to its new mission and goals. Significant features of the new program will include:

- **A plasma science initiative.** We will significantly increase funding for basic plasma science, beginning in fiscal year 1997. This initiative will broaden the academic base in the field through outreach to institutions not now involved in plasma science and will include a Plasma Science Young Investigator Award program. Over the next five years, fusion program funding for basic plasma science is projected to grow to approximately $10 million. This initiative will be coordinated with other agencies funding plasma science, such as the National Aeronautics and Space Administration and the National Science Foundation.

- **Increased emphasis on alternative concepts.** We will evaluate proposals to identify one or two small alternative concept experiments for construction. One that has already been favorably reviewed is the National Spherical Tokamak Experiment proposed by Oak Ridge National Laboratory, Princeton Plasma Physics Laboratory, and the University of Wisconsin to be built at Princeton Plasma Physics Laboratory. We will also continue our program of cooperation on large alternative experiments in Japan and Europe. We are asking the FEAC to recommend an overall strategy for conducting an alternative concepts research program in the United States, by this summer.

- **A changed but continuing role for Princeton Plasma Physics Laboratory.** The FEAC report recommends the shutdown of TFTR, after exploiting its scientific capabilities, but emphasizes the need to maintain the technical infrastructure developed through decades of investment at the Laboratory. This will provide the core competencies required for national leadership and international collaboration in fusion research.

o **The Department will shut down TFTR in either 1997 or 1998.** The TFTR, as the only tokamak now operating with deuterium-tritium fuel, offers the promise of unique advances in fusion physics if "reverse shear" operation inside the tokamak can be enlarged. By early 1997, we will have evaluated TFTR's work-in-progress, with the assistance of FEAC, and will either begin facility shutdown in fiscal year 1997, or will complete a focused experimental program leading to termination in fiscal year 1998. After TFTR shutdown, the Laboratory will continue to be a center of excellence in fusion and plasma science, with strong capabilities in theory and modelling, in diagnostics and instrumentation, and in experimental fusion science, although at a smaller scale and with an even greater emphasis on innovation than in the past.

o **No large facility construction mortgages.** With the termination of the Tokamak Physics Experiment, there are no longer any plans for large construction projects. Resources will be concentrated on increasing our fusion science knowledge base, construction of a limited number of innovative small experiments funded within an essentially flat budget, and an increased program of research in basic plasma science. Again, FEAC will recommend priorities among the research efforts that can be addressed using our facilities.

- **Leverage of program resources through international collaboration.** We will continue to leverage our resources through international collaboration, which is an integral part of every facet of the fusion program. We will also remain an active participant in the International Thermonuclear Experimental Reactor (ITER) Engineering Design Activities (EDA). The ITER is the only facility presently planned that will permit the study of the physics of long-pulse ignited plasmas, a key area of fusion science. We plan to be an active participant in the ITER EDA through its completion in July 1998. Failure to do so would damage our credibility as an international partner and specifically limit our access to facilities constructed by the much larger Japanese and European fusion programs. Continued participation in the EDA also maintains the option of participation in construction in a way consistent with U.S. program needs and financial limitations. Involvement in ITER construction would also allow U.S. industry to participate in the development of key fusion technologies at minimal cost. With these considerations in mind, the Department is beginning an interagency process that would lead to our participation in non-committal and informal discussions with our partners about ITER construction. As part of this approach, given the financial limitations, the Department will not seek to be the host of the ITER facility. In any event, international collaboration will continue to be integral to a restructured fusion energy sciences program.

- **Changed program governance.** The Office of Fusion Energy Sciences will be reduced in size and reorganized. The FEAC will be renamed the Fusion

Energy Sciences Advisory Committee (FESAC), and its membership changed to reflect the scientific nature of the program. FESAC will have a continuing Science Subcommittee that will play a role in providing scientific information to FESAC to aid in priority setting, and will serve as the focus of scientific leadership and consensus building within the fusion community. Peer review will be used for the evaluation of research proposals, assessment of progress and quality of work, and for initiating or terminating facilities, projects, and research programs or groups.

Management of the transition of this program to a world-class fusion energy sciences program will be one of the Department's greatest priorities in the coming year. While the broad outlines of the program are clear, many important decisions remain. I intend to seek the best scientific advice available in making those decisions, and will rely heavily on the outcome of reviews now being conducted by the Science Subcommittee of FEAC, in order to build community consensus around these decisions.

In closing, Mr. Chairman, I believe that this restructured program, built upon the carefully crafted FEAC report and the input of the fusion science and technology community, is responsive to the guidance provided by the Congress last year. It strengthens the fusion science and plasma science programs of the Nation, and broadens the knowledge base needed for an economically and environmentally attractive fusion energy source. This concludes my prepared testimony. I would be happy to answer your questions.

Chairman ROHRABACHER. Thank you, Dr. Krebs. And, I want to again express that we appreciate you coming and being here today.

We did have to work out scheduling problems. And, I also appreciate your spirit of cooperation.

You know, we are not used to this where we have Republicans in charge of the Legislative Branch and Democrats in charge of the Executive Branch. But, I really appreciate you and the other members of the Department trying to work with us and trying to make sure this thing functions well.

Dr. KREBS. It's mutual.

Chairman ROHRABACHER. Tom.

STATEMENT OF TOM SCHATZ, PRESIDENT, CITIZENS AGAINST GOVERNMENT WASTE, WASHINGTON, DC

Mr. SCHATZ. Thank you very much, Mr. Chairman. First, I want to congratulate you and the other Members of the Subcommittee for the fine work you did in fiscal year 1996.

I already testified last year before the Subcommittee and made several recommendations. And, you did follow through. And, we appreciate your making some tough choices, as you are again facing this year, in a time of great budget constraint.

I am certainly not an expert on fusion energy. You don't want to know how I did in physics in high school. I did pass. I did find it interesting.

But, I'm really here as a representative of the taxpayers and to hopefully raise some questions, bring some challenges before the Subcommittee so that you, as the people in charge of spending our money, and the experts in this field can get together and make some good decisions about how to proceed forward in this area.

The Grace Commission report on the Department of Energy supported controlled nuclear fusion as a possible source of peaceful energy. The Grace Commission report, of course, came out in 1984.

And, at that time, they said, "The fundamental difficulty in producing fusion reactions in either a laboratory or power plant is that no physical container can withstand the necessary temperatures for an adequate length of time." I think that's still true today, even though, as the previous panel testified, some progress has been made.

Dr. Park of the University of Maryland has often said or did say that everyone said fusion was 10 or 20 years away. Now, it's 40 or 50 years away. It's one of these questions that really does need to be addressed in terms of planning the future of energy research in this country.

The TFTR, we recommend that it be eliminated now, not 1997 or 1998. A few things get done and then they say 1999, 2000, 2001, 2002.

There is nothing more elusive, I think, in Congress than making a firm decision about when to actually end a program. I was looking at something totally unrelated to this area, something called rice modeling in conjunction with our examination of appropriations bills.

And, at the end of a description of this project, it said, "It's difficult to say when such a project is finished unless a specific time frame or milestone is established in advance." And, it seems that that often is a moving target here on Capitol Hill.

We found some very positive recommendations in the FEAC report, as Dr. Krebs testified, increasing plasma research, looking at the alternatives. We think that makes a lot of sense.

Some of the smaller projects often get swallowed up in the larger projects in science. That's one of the reasons we have looked at projects like the Space Station, like the SSC—nothing personal.

But, we do think that there may be a longer-term benefit and a greater benefit to the taxpayers if we put more money into these smaller projects. And, then we can see whether or not they will actually bear fruit in the future.

We also agree that the internal personnel and programs at the Princeton Plasma Physics Lab are important and that it would be somewhat of a loss to taxpayers to simply shut it off and throw everybody out the door. We are not suggesting that in any way.

In terms of General Atomics, the DIII-D program, $38 million last year, it's our view that this is a commercial enterprise at some point and that it should be examined very closely, the feasibility of getting the private sector involved at some point in the future.

Interestingly, in one of the Grace Commission's recommendations, it was to privatize the Space Shuttle, which everyone at the time thought made no sense. Last year, the GA had subcommittees that started to take a look at that in appropriations.

So, some of these things get suggested. They should be looked at and really looked at in terms of who will benefit ultimately from these kinds of projects.

The same applies with MIT. We recommend there be simply a 25-percent cut and encourage MIT to find alternative sources.

We give them five years again. These are suggestions. We are open to alternative suggestions. But, the concept, we think, is very important.

In terms of ITER, we believe that funding should be frozen. The prior panel recommended that we go through the design phase.

It seems to be an awful lot of questions about how that will eventually work out. Certainly, the U.S. should not take the lead. I think everyone is pretty much agreed on that.

But, we have questions about the commitment of other countries over a 30 year period to this kind of technology. And, really it's the same question we are asking about the Tokamak. Who is going to look at this and say, "We will commit X dollars for the next 30 years," for something we are not even sure really will work at that time?

The U.S. is clearly at a crossroads in fusion research and certainly setting priorities. Researchers, we believe, need to know three things before a reactor gets developed—what is the best fuel source, best way to contain the reaction and the best way to convert that reaction to affordable energy.

We don't have answers. We have a lot of good questions. We think that working on the development is really putting the cart before the horse at this point.

We, again, believe that fusion is an important key to our future. As non-sustainable fuels continue to be used at accelerating rates, the need for alternative energy becomes greater. And, fusion, of course, is not the only alternative in the big picture in the science and energy budget.

We recommend a funding level of $200 million. There are obviously numerous ways to get there. Eliminating TFTR right away, reducing the international commitment and still allowing these alternatives to proceed, we believe, should be considered at this time.

That concludes my oral statement, Mr. Chairman. I appreciate your putting the written statement in the record.

And, I would be happy to answer questions.

[The prepared statement of Mr. Schatz follows:]

Testimony of
Thomas A. Schatz,
President,
Citizens Against Government Waste,
before the
Energy and Environment Subcommittee on Science
March 7, 1996

Good morning, Mr. Chairman. Thank you for the opportunity to testify today before the Environment and Energy Subcommittee on Science. My name is Tom Schatz and I represent the 600,000 members of Citizens Against Government Waste (CAGW).

CAGW was created 11 years ago after Peter Grace presented to President Ronald Reagan the 2,478 findings and recommendations of the Grace Commission (formally known as the President's Private Sector Survey on Cost Control). These recommendations provided a blueprint for a more efficient, effective, less wasteful, and smaller government.

Since 1984, the implementation of Grace Commission and CAGW recommendations has helped save taxpayers more than $430 billion. CAGW has been working tirelessly to carry out the Grace Commission's mission to eliminate government waste, but with the national debt approaching $5 trillion our work is far from done.

Mr. Chairman, you and the members of this subcommittee face one of the most important tasks confronting our country -- reforming Washington's wasteful ways.

As the 104th Congress enters its second session, two objectives should be met: sustaining the revolutionary reforms that have already been passed and identifying further unnecessary spending to cut. Doing this has not been easy, nor popular, but it must be done.

The national debt and chronically high budget deficits are clear indications that we have been living beyond our means. Unfortunately, spending must be drastically reduced, programs eliminated and priorities re-ordered to ensure a sound economic future. If taxpayers cannot get a real return on their investment, then the program should be eliminated. If a program is not significantly time-sensitive, it should be deferred until the budget is balanced.

First, I would like to congratulate you, Mr. Chairman, and the members of this subcommittee for the fine work you did for fiscal year 1996. As you may recall, I testified before this subcommittee last year, and your work met or exceeded most of the challenges we laid out. Not only did you talk the talk, but you walked the walk. CAGW hopes that other subcommittees follow the example you have set.

Today, I come before you to testify on the FY 1997 fusion energy budget. Let me begin by saying that I am not here as an expert on fusion energy, but as a representative of the taxpayers, who want a real return on the investment of their hard-earned tax dollars.

The Grace Commission report on the DOE touted controlled nuclear fusion as "a possible source of peaceful energy." The report added, however, that "the fundamental difficulty in producing fusion reactions in either a laboratory or power plant is that no physical container can withstand the necessary temperatures for an adequate length of time." Mr. Chairman, after 40 years of research on fusion energy, science tells us that our Sun is the only "container" yet found that can fuse and release such vast amounts of energy.

No one can argue that fusion energy will be a viable energy source of the future, but research has netted few results. Before committing billions of taxpayer dollars to questionable programs such as the Tokamak Physics Experiment (TPX) or the Tokamak Fusion Test Reactor (TFTR), more research must be done to provide a better understanding of how best to harness fusion energy.

Even after significant cuts to the fusion budget in FY 1996, the Tokamak-based fusion power reactor still dominates the budget and threatens the development of potentially better programs. Mr. Chairman, we are putting all of our eggs in one basket. Allowing one particular program to crowd out others or inhibit alternative fuel usage reactors can only serve to delay discovering a successful fusion reactor to develop.

The TPX was intended to be the second generation of Tokamak reactors. The project has already cost taxpayers more than $10 billion and would have cost another $2.2 billion in the next five years without your efforts to prevent such expenditures. Experts contend that this technology will be neither operational nor commercially viable until the year 2040, and the final cost may be $30 billion. Even if a successful program was developed, other experts believe that the output will not meet acceptable standards. We applaud your decision to end the TPX in the FY 1996 Budget and encourage you to end funding for other Tokamak-based programs.

Scientists are questioning the fundamental viability of the Tokamak reactor. Dr. Lawrence Lidsky, a nuclear engineer at MIT, has said that "[b]y focusing on the Tokamak, we're committing to a technology that is a commercial nonstarter. And shifting gears to another design down the road won't be easy." While taxpayers have already lost $10 billion on this project, we must cut our losses and get out.

Ed Rodwell, a manager of Advanced Nuclear System at the Electric Power Research Institute said that "[s]cientists have scarcely begun to work on the basic engineering problem of converting fusion energy to electricity. We don't invest in it [fusion] -- it's too far away. There could be a massive long-term benefit, but to get from here to there is going to take a hell of a lot of money."

Tokamak research receives roughly sixty percent of the fusion budget, crowding out other research and development. Since the advent of the Tokamak reactor, no one has been able to produce more energy than the amount consumed. While CAGW does support research in the fusion energy field to effectively create a cheap, constant, and safe source of energy, the Tokamak project is doing more to inhibit than to advance such research.

The U. S. Office of Technology Assessment's February 1995 report concluded that "the most optimistic supporters of fusion note that many scientific, engineering, and economic challenges remain to be met...[A]bsent novel, unexpected science developments, progress toward development of a fusion power plant would require a commitment to construction of expensive new facilitates."

Scientists are stymied by which path to follow and how soon a successful program can be started. Dr. Robert Park, Physicist at the University of Maryland has said, "For years, we joked that fusion was 20 years away and always would be. Now most scientists say it is 50 or 60 years away...What fusion needs is a breakthrough, and that hasn't happened...Maybe it will pan out, but it is surely a long way away."

Therefore, science must return to the basics on fusion. The first priority is to determine which fuel should be used and how can it be contained. There are experts here to offer suggestions for alternatives to the deuterium-tritium fusion proposals. Then we must be able to successfully convert that fusion into usable energy at a commercially acceptable rate. Then and only then should we commit ourselves to developing models in which to produce this energy.

Researchers may protest that fusion progress will be set back for years if funding is not increased for FY 1997. But progress has already been seriously hampered because of incorrect priorities in the fusion program.

One suggested alternative to U. S. efforts is the International Thermonuclear Experimental Reactor (ITER), a collaborative effort by the U. S., the European Union, Japan, and the Russian Federation. The total cost of the ITER construction is estimated at $10 billion and the U. S. portion of the design phase is approximately $300 million. But the ITER will not be able to generate electricity. That will come with the Demonstration Fusion Powerplant reactor (DEMO), which will not be ready until 2025, and a commercially viable design probably won't be ready until 2040.

While CAGW applauds the efforts to join our international allies in developing worthwhile energy programs, taxpayers cannot be asked to follow them on a wild goose chase. As with many other international development efforts, the ITER program will promise much and deliver little. Moreover, utilities experts doubt that any of the commercial models developed from the current field of prototypes would be commercially attractive and would be able to meet usage demands. CAGW also questions whether all contributor nations involved can maintain their commitment for the next 30 years.

I would like to turn my attention to the Department of Energy's Fusion Energy Advisory Committee's (FEAC) recommendations in their January 27, 1996 report, *A Restructured Fusion Energy Sciences Program.* FEAC endorsed a $275 million funding level for FY 1997, but offered $250 million as an acceptable alternative. FEAC has projected an apocalyptic scenario of the future of fusion, if funding is cut. CAGW does not agree.

We do agree with some of the recommendations made in the report. Increasing funds for plasma science and other alternatives would encourage further research into an optimal fusion development program. Moreover, the Princeton Plasma Physics Laboratory (PPPL) would be the ideal candidate to take the lead because established programs and personnel would save costs and time. CAGW also supports FEAC's recommendations for a modest increase in materials in technology and DOE reductions in program office staffing. We also support an extensive range of new experiments and a robust theory and modeling program of non-Tokamak technology.

CAGW has several recommendations that we feel will better redirect and restructure the fusion program. First, only funding for termination for the Tokamak Fusion Test Reactor at PPPL should be allowed in the FY 1997 budget. To date, the TFTR has yielded little results, and we are concerned that the shortfalls of the Tokamak program would only serve to further drain taxpayer resources.

Last year, Congress funded the General Atomics' (GA) DIII-D at $38 million. However, GA should shoulder the costs of this program, if it is to be commercially viable, since the company will benefit most from its development. Our recommendation is that this subcommittee begin a phase out of taxpayer funding of the DIII-D, over no longer than three years. This would give GA enough time to prepare for the full responsibility of maintaining the program.

In FY 1996, Congress funded MITs Alcator C-MOD reactor at $10 million. Unlike the DIII-D at GA, MIT will most likely find difficulty in getting private funding to support their reactor. CAGW recommends a 25 percent cut in funding for FY 1997, and to encourage MIT to find alternative sources to maintain the program, for no longer than five years.

While CAGW supports international development projects that will lessen taxpayer investment on shared benefit technology, further evaluation of the ITER program is necessary. CAGW recommends that the subcommittee freeze ITER funding at FY 1996 levels and prevent any funding to go towards reactor construction. No funding should be allowed after 1998, unless all our international partners have maintained their obligations. But if at all possible, further international funding should be focused primarily on basic fusion research.

The question becomes, "Where do we go from here?" I realize it is a scientific impossibility to ask researchers to take steps backwards, but that is what we have to do. Many experts believe that at this point, costs would be too significant to develop a commercially viable reactor. Even if it did, there are even more fundamental problems, such as the most effective way to convert fusion to electricity.

The United States is at a crossroads in fusion research. Before any reactor can be developed, researchers should first know three things: what is the best fuel source, the best way to contain the reaction and the best way to convert that reaction to affordable energy. So far, not one of these questions has been answered. Constructing a questionable reactor before addressing these fundamental questions is putting the cart before the horse.

Mr. Chairman, expert after expert has expressed grave doubts about the short-term potential of developing a successful fusion program. We have also noted that you have expressed grave doubts about the future of the fusion program. Fundamentally, fusion is an important key to our future. As non-sustainable fuels continue to be used at accelerating rates, the need for alternative energy becomes greater.

Although FY 1996 funding was $244 million, CAGW's recommends that FY 1997 funding should not exceed $200 million. Instead of throwing away taxpayer dollars, Congress can begin an evaluation of fusion research and the direction it should take. We do not believe that this type of spending reduction will seriously hamper fusion research and may in fact save taxpayers more money in the long term.

Before approving the expenditure of one more tax dollar on programs under your jurisdiction, members of this subcommittee should ask themselves two questions: (1) is this project worth the further weakening of our representative government?, and (2) is this a project that I want my children and grandchildren to be responsible for paying?

We are asking that before one more precious dollar is needlessly wasted, you carefully examine whether the project needs to continue. While considering authorization bills this year, we urge you to keep national interests, not local interests, in mind.

Congress usually has good intentions when spending money, but the power to do good is also the power to do mischief with our tax dollars. Care must be taken when looking at individual programs and projects; the more open and honest you are, the more likely a project is to withstand the light of day.

People want their power back. You can make a difference. Discretionary spending is one-third of the federal budget; it's real money. It's time to stop taking our tax dollars and start making tough choices.

Taxpayers are no longer amused by inadequate and irresponsible management of our government because their future is in jeopardy. The budget crisis cannot be ignored, and that's why their amusement has been replaced with outrage. Members of this subcommittee must be equally as outraged.

Restoring fiscal sanity to our nation is the most important job for the 104th Congress. The country is awash in a sea of red ink, and every day slips perilously closer to bankruptcy. The national debt is expected to rise to nearly $6 trillion by the end of the century. This is not the legacy that we should leave to our children and grandchildren. Spending has not been cut to the bone. Money is being wasted daily and the clock is ticking. We're sitting on a fiscal time bomb that needs to be defused.

You have an opportunity to continue the mission that Peter Grace and Ronald Reagan started 13 years ago when President Reagan signed Executive Order 12369 in 1982 formally establishing the President's Private Sector Survey on Cost Control.

This concludes my testimony. I'll be glad to answer any questions you may have.

Chairman ROHRABACHER. Yes, without objection, it will be put into the record.

And, Mr. Gavin—not Galvin, but Gavin.

STATEMENT OF JOSEPH G. GAVIN, JR., RETIRED FROM GRUMMAN AEROSPACE, AMHERST, MASSACHUSETTS

Mr. GAVIN. Thank you very much, Mr. Chairman. I do appreciate the opportunity to express my views.

I should explain to you that I am a member of FEAC, the Fusion Energy Advisory Committee. I was also a member who could not endorse the current report.

Now, let me say that the report is an excellent report. It answers the charge given it by the Department.

My concern is that the Committee should have gone back to the leadership of the Department and asked them to do more and to be more aggressive. To make it short, I believe firmly that the PCAST recommended funding of $320 million is more appropriate.

I should define that. The level of funding in the Department's charge, all of them, to me represent conceding now that we have become third rate in this particular branch of science and engineering.

The $320 million that PCAST recommended, I think, represents a holding position where we perhaps will not be the leaders but we will not be so far behind that we cannot at some point catch up if it seems the thing to do. And, the reason I feel strongly about this is that, although I don't expect to see it in my lifetime, fusion energy will be a commercially-important technology and one that we should sell, not buy. And, I think that this is what separates us from the Europeans and the Japanese at this point, this consideration of the future commercial potential.

Now, it's very interesting to look back over the past. In the 20s and 30s, Europe was the leader in science and technology.

We spent a tremendous effort of playing catch-up during World War II. And, World War II and the Cold War that followed caused a government investment in research and development in this country which has made us the leaders in the world.

And, I've had enough experience overseas to understand how this has come about. And, I understand quite clearly how they are now trying to displace us from that leadership.

I'm just hitting the high points in my written submittal. I think it's worth nothing that the Americans are pretty casual about the availability of energy. We've rarely had an interruption.

Some of us remember that during World War II we had gasoline rationing. But, you know, we stopped worrying about that until the oil embargo in the 70s. And, that has been forgotten, too.

And, of course, the Nixon Administration promoted energy independence. But, with time, that initiative died.

Now, more recently, we fought a pretty vicious war to keep Saddam Hussein from seizing control of the Gulf reserves. And, we take it for granted.

We are importing more than half the oil we use. And, that represents about 40 percent of our trade imbalance.

We are using petroleum products at a third to a quarter of the consumer prices paid by those who are our economic competitors.

And, frankly, we have no economic incentive to make the necessary investment to improve our efficiency. We use the petroleum products much less efficiently than our competitors.

Now, the reason for being concerned about energy is that the demand for energy in the future is not just a domestic problem. It's a global problem.

And, it's quite clear that we do not, in many cases, take the long view which takes into account the growth in population and the growth in urbanization, which amplifies the demand for energy. So, what we are talking about in the long run is promoting political and economic stability on a global scale.

Now, in my written presentation, I speculate about why fusion research, in my view, is on the brink of fiscal euthanasia. I won't go into that here.

I would point out, however, that the Europeans have increased their investment in fusion research. I think Chairman Conn pointed out to twice our level. And, the Japanese, some 50 percent higher than ours.

And, we have not involved industry to the degree that our competitors have. They have recognized the potential of a high-value, high-technology industry with a global market.

I would like to wind up by summarizing the position I took within FEAC as to what I think we should have recommended. I think that we should have asked the leadership of the Department to make a much more aggressive bid to achieve the PCAST recommended funding of $320 million.

I think that, with respect to ITER, we should complete our commitment on the engineering design phase. We do not have a very good reputation for carrying out international agreements. I would not like to see us welch on this one.

As far as the construction phase is concerned, I think that there is no question that the budget realities require that we do not have a major role. And, I think it has been pointed out by several others that perhaps there is a subsidiary role that we can develop which will allow us to have some participation. But, I think the idea of having equal participation has gone by the boards.

I think we should have—the FEAC Committee should have argued more strongly for a continuing use of the three major facilities—the tokamaks that we have. They are all different. They are all unique.

And, they all have more that can be constructively done for at least a couple of years to come. I would think that the TFTR should perhaps be the first one to be mothballed.

I think that we should undertake something——

Chairman ROHRABACHER. Mr. Gavin, perhaps you could go to your central last point, because we have to move on to the next panelist.

Mr. GAVIN. Yes, I understand. I am in agreement in pursuing one or two alternative confinement schemes.

And, I think because of the fact that we will not be involved in the construction of ITER that at some point, perhaps several years downstream, serious consideration should be given to a new reactor—I'm talking of a Tokamak type reactor—which might be a derivative of a TPX or an NSTX, but something that would provide in this country the ability to conduct an ignition experiment.

I would also recommend that some consideration be given to producing some stability in the program. Instability does not attract industry. And, it certainly does not attract young people, the talent we need for the future.

And, to make one last point, we are talking about an increment of less than $100 million. And, you asked, Mr. Chairman, where would I find that money. And, I have some suggestions.

Chairman ROHRABACHER. I definitely want to hear this.

[Laughter.]

Mr. GAVIN. If I take a round number like $100 million, which is more than enough to accomplish the PCAST funding, that represents one-fourteenth of one B-2 stealth bomber. It represents less than two fighter aircraft. And, it represents one-tenth of one cent on the gasoline tax.

Now, the program in fusion in this country has had too many successes to give out at this point and take a third rate position.

Thank you very much for your attention.

[The prepared statement of Mr. Gavin follows:]

Statement by

Joseph G. Gavin, Jr.

to the

Subcommittee on Energy and Environment
Committee on Science
U.S. House of Representatives

concerning

DOE's Restructured Fusion Energy Sciences Program

March 7, 1996

INTRODUCTION

I appreciate very much the opportunity to offer my views to your committee. As a member of the Fusion Energy Advisory Committee (F.E.A.C.), I found myself unable to endorse the recent report of that group I wish to be clearly understood in this matter. The committee responded to the Department of Energy's charge; I was and am now convinced that the committee should have challenged that charge. What I have to say today is based on a position paper that F.E.A.C. did not find compatible with its report.

There are four points that I wish to make before detailing the recommendations I made in my position paper submitted to F.E.A.C.:

1) We have not had a consistent far-sighted national energy policy and we need one.

2) Energy is a vital and dominant factor in our present, our future, and our national security.

3) The Department of Energy has much that not only deserves support but also warrants more aggressive execution.

4) We are no longer in a Cold War; we are in a hot, fiercely contested international economic struggle that we cannot afford to lose.

The remainder of my comments consists of my January recommendations to F.E.A.C. In these recommendations I have taken the "long view" based on personal experience.

RECOMMENDATIONS TO F.E.A.C.

The D.O.E. (Department of Energy) charge to the F.E.A.C. (Fusion Energy Advisory Committee) in December 1995 represents fundamentally flawed policy. The request for program recommendations tailored to several funding levels from $200MM to $275MM per year will result in funding in the $220MM range at best. This would represent a significant reduction from the current $244MM, which in turn was a dramatic reduction from the July 1995 recommendation of $320MM made by P.C.A.S.T. (President's Committee of Advisors on Science and Technology). Accepting funding in the $220MM range is an acknowledgement that the United States has conceded to Europe and Japan clear leadership in the development of fusion energy, a technology that has been considered for many years to be vitally important to the global energy future. Fusion energy will be a commercially important technology—one that we should sell rather than buy,

I am convinced that this is such as important issue that this committee (F.E.A.C. should answer the D.O.E. charge in a manner that considers the broader aspects of today's situation and that forcefully argues for higher priority for the fusion program. My conclusion, to jump ahead, is that the Secretary of D.O.E. and the Administration must make a determined effort to obtain the P.C.A.S.T. July 1995 recommended funding level of $320MM This is not a matter of affording it; it is a matter of priorities.

Let us review the past to gain perspective. In the 1920s and 30s Europe was the leader in science and technology A promising undergraduate in the United States looked forward to graduate years in England or Germany. World War II intervened. The war provided a tremendous stimulus to federally funded research and development in this country. This government investment produced remarkable progress. After the war, as the nations of Western Europe turned their attention to rebuilding. the undamaged United States forged ahead following the directions embodied in Vannevar Bush's *"Science—The Endless Frontier."* The ensuing Cold War confrontation with the Soviets provided a continuing motivation for some 40 years of U S. government support for a wide variety of scientific and engineering endeavors.

Energy was not in the headlines after World War II and people forgot wartime gasoline rationing. In the 1970s, however, the "oil embargo" did refocus our attention on the supply of energy At about the same time, the public's awareness of atmospheric pollution began to grow The Nixon administration promoted "energy independence" but, with time, this initiative died.

More recently this country fought a brief, vicious war to prevent Saddam Hussein from seizing control of the Gulf oil reserves. Domestically we are quite comfortable, enjoying petroleum products at 1/3 to 1/4 the consumer price paid by our economic competitors. We import more than half the oil we use although it represents about 40% of our trade imbalances. In industry, we use energy measurable less efficiently than the Europeans or the Japanese. We have no economic incentive to make the investment necessary to improve our efficiency.

The P.C.A.S.T. report in July 1995 confirmed once more that fusion represented one of the very few attractive sources of energy for the long term. Those arguments will not be repeated here; however, it is important to recognize that the demand for energy in the future is not just a domestic problem—it is a global one. The aspirations of the underdeveloped countries, the growth in population, and, what is often overlooked, the growth in urbanization all insure a continuing growth in the demand for energy. Satisfying this demand will be an essential factor in promoting political and economic stability on a global scale.

With the above background, it is worth examining why fusion research in the United States is on the brink of fiscal euthanasia. In the mid 1980s the United States was arguably the leader in many aspects of fusion research. Through three Republican administrations and now in a Democratic administration, the federal investment in fusion research has been progressively curtailed in spite of periodic successes. The reasons may be as follows:

1. Commercial power generation from fusion is years in the future. Over the past several decades there has been a growing focus on the immediate future at the expense of the long term. This is apparent in politics, industry, business, and in people's lives and attitudes.

2. A convincing "ignition" demonstration has not yet provided assurance to people outside the fusion community that fusion's potential is really achievable. The international collaboration called I.T.E.R. (International Thermonuclear Experimental Reactor) is focussed largely on this point.

3. The merit of federal funding of research and development has been generally questioned. It is more favorably viewed where the Department of Defense is concerned; less favorably for D.O.E. even though national interest and security are clearly involved. A report by the Council of Economic Advisors dated October 1995 concludes that federal funding of research and development has been a major factor in stimulating our economy and building our leadership in many technologies.

4. The cohort of younger congressmen and their staffs has no first-hand recollection of the 1920s, 1930s, World War II, or even the early years of the Cold War. Consequently their views cannot have been influenced by the experience that has "tempered" my generation

The United States made a commitment to join Europe, Japan, and what was the Soviet Union in the engineering design phase of the proposed International Thermonuclear Experimental Reactor (I.T.E.R.). As we have cut our funding for fusion, the Europeans have increased their investment in fusion research to a level twice ours; and the Japanese have increased to a level 50% higher than ours. They have involved industry to a degree that we have not. They have recognized the potential of a high value, high technology industry with a global market.

In summary, I recommend strongly that the report of this committee (F.E.A.C.) should include:

1. A forthright challenge to the Secretary and to the Administration to undertake a determined effort to convince Congress of the merit of providing annual funding for fusion at the P.C.A.S.T. recommended level of $320MM.

2. A recommendation to the Secretary to carry out our commitment to the I.T.E.R. engineering design activity and to notify our partners that the United States cannot be a major partner in any I.T.E.R. construction program. We have no means to make such a long time, expensive commitment.

3. An argument for continuing support and operation of our major fusion facilities, T.F.T.R, D.I.I.I.D. and Alcator C-Mod. All can be used constructively for several years to come.

4. A plan for selecting one, or at most two, alternate plasma confinement methods for further study and possible development. This undertaking should complement rather than duplicate what is being done abroad.

5. A recommendation to select a TPX-like or NSTX-like device to be designed and eventually to be constructed to replace T.F.T.R. The progress and timing should be tailored to take advantage of results for (3) above and to minimize budgetary impact.

6. An argument for a stable, predictable program. Instability will not attract industry participation or the talent needed for the future.

7. A blunt rejection of lower funding levels. Programs planning to accept lower funding level is an invitation to marginalization of fusion research in the United States.

These recommendations, seeking $100MM to achieve funding at the P.C.A.S.T. $320MM level, represent 1/14 of one B-2 stealth bomber, less than 2 fighter aircraft, or 1/10 of one cent on the gasoline tax!

The fusion program in the United States has produced too many successes and has too much promise to give up without a fight, without serious review of national priorities. It will require leadership, not poll-following

J. G. Gavin, Jr.
Member, F.E.A.C.
18 January 1996
Minor revisions, 4 March 1996

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S.B. and S.M. in Aeronautical Engineering (1942).

Public Latin School, Boston, MA

POSITIONS HELD

1946-1990 Grumman Corporation
Design Engineer 1945-1950
Project Engineer 1950-1962
Vice President and Director of the Lunar Module (Apollo) 1962-1972
President of Aerospace Subsidiary 1972-1976
President, Chief Operating Officer and Director 1986-1985
Consultant 1986-1990

1942-1946 Lieutenant Senior Grade, USNR

HONORS, AWARDS

Tau Beta Pi, 1939
Sigma Xi, 1940
NASA Distinguished Public Service Medal, 1971
American Institute of Aeronautics and Astronautics
Member - 1946, President - 1982, Honorary Fellow - 1984
National Academy of Engineering, 1974
International Academy of Astronautics - Member 1986
American Association for the Advancement of Science
Director - 1988-1992, Fellow - 1991

OTHER

Massachusetts Institute of Technology
Life Member of Corporation - executive committee, 1984-1991
President of Alumni/ae Association, 1986

National Research Council
Chaired committees on: "Cooperation and Competition on the Path to Fusion Energy" 1984; "Advanced Space - based High Power Technologies" 1989; "From Earth to Orbit" 1992

OTHER (continiued)

Energy Research Advisory Board
    Member - 1984-1989
    Chaired: "Report of the Technical Panel on Magnetic Fusion of the Energy Research Advisory Board" November 1986

Huntington, New York Hospital
    Member of the Board of Directors, 1975-1991

European American Bank - New York, New York
    Director, 1976-1985

PERSONAL

Date of Birth: September 18, 1920
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Chairman ROHRABACHER. Thank you. Thank you, Mr. Gavin.

Mr. Adams.

STATEMENT OF JAMES ADAMS, SENIOR ANALYST, SAFE ENERGY COMMUNICATION COUNCIL, WASHINGTON, DC

Mr. ADAMS. Thank you very much. Good afternoon, Mr. Chairman and Members of the Subcommittee.

One of the things that strikes me is that I think there is general agreement that the fusion program needs to be restructured. The question is, how do we go about doing it?

The Safe Energy Communication Council is a non-profit energy watchdog coalition of national energy, environmental and public interest organizations. We, in principle, support the idea of developing clean, affordable fusion energy.

Unfortunately, the DOE Fusion Energy Program has been completely dominated, in our view, by tokamaks. And, I think this is really the fundamental problem.

I agreed with the statements of Dr. Miley. I was pleased to see you bring up Dr. Lidsky's letter. And, I think that is essentially the nub of the matter.

For fusion to succeed in the near term and the long term, I do believe we have to focus on the alternative concepts. And, during the last two decades, unfortunately, the focus has been primarily on tokamaks. And, the alternative concepts have really just been left by the boards.

You mentioned, Mr. Chairman, that we have spent $14 billion in 1995 money in 40 years. If we were to continue on the tokamak path that was discussed in last year's budget, we would probably spend somewhere between $25 billion and $30 billion. We think this is unacceptable, given the budget constraints that we are in nor will it lead to affordable energy.

If, in fact, it takes to the year 2040 before we have a commercial reactor, that will mean that taxpayers have been subsidizing fusion research for approximately 100 years.

I wanted to—I submitted a letter to the staff from Mr. David Montgomery, who has been a fusion researcher since the 1950s. I won't read the whole quote but, in essence, he agrees that the fusion program has gone off track, that the tokamak has dominated it and that the alternatives have systematically been ignored. And, I think that is the thrust of Dr. Lidsky's comments as well.

In a time of tight budgets, U.S. taxpayers cannot afford such an expensive and speculative research program. And, in fact, for the fiscal 1996, approximately 60 percent of the fusion budget, $154 million, relates to tokamak reactor. One of our main concerns is that the funding for the tokamaks competes with other energy resources such renewables. And, the entire program for renewables for 1996 is $275 million and the fusion program is $244 million.

This creates a tension between these two programs. And, in our view, renewable energy resources such as solar and wind and geothermal are much more cost effective and are much closer to being affordable.

Indeed, wind right now provides energy at approximately five cents a kilowatt hour in some places. And, solar is coming down in that range as well. The point being that if DOE is going to assert its priorities, we think that the focus on tokamaks keeps them from really funding energy resources that are more cost effective in the long run. And, that's why we think that should be DOE's number one priority, not tokamak dominated fusion.

This is actually something that was raised in the Bush Administration in 1991. They did a thorough study in the ranking of 23 energy technologies. Out of those 23, fusion was ranked 22nd, 22nd out of 23. I believe it was the Alvis program. I am not sure on that.

But, I know that fusion was the 22nd. And, yet the funding for fusion was kept at relatively high levels.

One of the problems is also that utilities have shown very little interest in fusion. The problem is that the Electric Power Research Institute decided to close its fusion program back in 1983, because they did not see fusion leading to the type of cost-effective energy that utilities would find useful.

To sum up, Mr. Chairman, I would like—we have made some specific recommendations. I won't go into detail of all of them.

But, generally they refer to, specifically, that we believe TFTR should not operate after this fiscal year. We agree that the Alcator facility, which is run at MIT, should start investigating alternative funding resources.

We also think that the General Atomics facility should investigate privatizing. It's not that we want to see these facilities shut down. We just think that the taxpayers cannot afford to fund three tokamak reactors and investigate a fourth on an international basis. And, that is our recommendation for that.

We also agree that the Department should fund at least 10 percent of its budget for basic plasma physics and another 10 percent for alternative fuels such as helium that Dr. Miley mentioned earlier today. We really think that that is the more promising path for fusion to go on. And, we would encourage that.

Thank you for the time.

[The prepared statement of Mr. Adams follows:]

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TESTIMONY OF

JAMES ADAMS
SENIOR ANALYST
SAFE ENERGY COMMUNICATION COUNCIL

Before The

SUBCOMMITTEE ON ENERGY AND ENVIRONMENT

COMMITTEE ON SCIENCE

U.S. House of Representatives

March 7, 1996

Good morning Chairman Rohrabacher and members of the Subcommittee. I am James Adams, Senior Analyst for the Safe Energy Communication Council (Council). The Council is a non-profit energy watchdog coalition of national energy, environmental and public interest organizations working to increase public awareness of the ability of energy efficiency and renewable energy resources to meet an increasing share of our nation's energy needs, and of the serious economic and environmental liabilities of nuclear power.

I am pleased to present the Council's comments and recommendations regarding the U.S. Department of Energy's (DOE) Fusion Energy Program and in particular, the report recently released by the Fusion Energy Advisory Committee (FEAC) entitled "*A Restructured Fusion Energy Sciences Program*".

In principle, the Council supports the concept of developing clean and affordable fusion energy as a component of a broader sustainable energy program based predominately on renewable energy resources and energy efficiency technologies for the 21st century. Unfortunately, the DOE fusion research and development (R&D) program has become narrowly focused on a research track fixated on burning tritium fuel in tokamak reactors. Based on the information to date, tokamaks are extremely expensive, scientifically unproved, technologically challenging, and would generate significant amounts of radioactive waste. After 40 years and $14 billion in constant 1995 dollars of taxpayer funded research, the DOE admits that it will be more than four decades before a commercial power plant is available.[1] At that point, taxpayers will have subsidized fusion research for almost a hundred years without any guarantee that it will be commercially viable.

During the last twenty years, the fusion program has narrowed its focus to tokamak technology to the detriment of basic plasma physics and alternative concepts. A number of fusion researchers have criticized this development. According to Dr. David Montgomery, Professor of Physics at Dartmouth College, a fusion researcher since the 1950s:

> "...the [fusion] program has gone off track in a serious way over quite a long period of time...the tokamak partisans completely ran away with the show...the American program was sharply re-directed from top to bottom...one by one the alternative approaches were hunted down, isolated, and either turned off or manipulated into tokamak support groups...basic

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[1] "Energy Supply Research and Development", U. S. Department of Energy, FY 1996 Congressional Request, Volume 2, February 1995, p. 468.

plasma physics was treated as a decoration to the field."[2]

In a time of tight budgets and reassessing government spending priorities, U.S. taxpayers cannot afford such an expensive and speculative research program. The Council understands that this was a major reason why the U.S. Congress reduced funding for fusion by over 30 percent for FY 1996 and ordered the Department to prepare a report on how the fusion program should be restructured. However, DOE's tokamak related funding for FY 1996 still comprises more than 60 percent ($154 million) of the fusion program.[3]

The large amount of R&D money spent on fusion, primarily related to tokamaks, competes with funding for renewable energy resources that are more cost-effective and have a much greater chance of providing energy in the near term. For example, federal support for photovoltaic programs that are cost-shared with the private sector has led to the largest increase in manufacturing capacity in the history of photovoltaics with American industry leading the way.[4] This is a wise investment that is paying off now by creating new high-tech jobs, enhancing America's competitiveness abroad, and will soon be providing clean electricity.

Despite this and other examples, DOE funding for the entire renewable energy budget (including solar, wind, hydrogen, geothermal and biomass) for FY 1996 is only $275 million while fusion is allocated $244 million. The Council believes that excessive funding for tokamak-based fusion is disproportionaly high in comparison to the numerous and diverse renewable sources available and creates an ongoing tension between the two programs for scarce federal dollars within the energy R&D budget.

DOE Energy Priorities

The issue of priorities within the DOE energy R&D budget must be addressed as well. The bias toward fusion funding has continued despite the fact that under the Bush Administration, DOE acknowledged that fusion did not merit the disproportionate federal subsidies it receives but did nothing to alter the imbalance. In a July 16, 1991 memorandum to then Energy Secretary James Watkins, Linda Stuntz, then Director of DOE's Office of Policy Planning and Analysis, reported the rationale and the results of the Policy Office's ranking of DOE's R&D priorities. Explained Stuntz: "Altogether, this package reflects the results of a considerable effort to develop, on the merits, program planning priorities in tune with the [National Energy Strategy]. Political sensitivities can be applied later, but you need to know, first, what seems to be right based on the merits, determined in accordance with criteria carefully selected and applied as uniformly as humanly possible across all relevant program elements."[5]

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[2] Letter from Dr. David Montgomery to James Adams, SECC, November 20, 1995.

[3] "Fusion Budgets", Office of Fusion Energy and Energy Research, The U.S. Department of Energy, January 2, 1996.

[4] "Testimony by Scott Sklar, Executive Director, Solar Energy Industries Association", Before The Committee on Appropriations, Subcommittee on Energy and Water Development, February 29, 1996, pgs. 1-2.

DOE's Policy Office methodology involved assigning, ranking and comparing variables of 23 energy technology options in three broad portfolios: oil vulnerability, electricity and basic sciences. The evaluation criteria included total quads of energy produced, national security, economic efficiency, environment, technical risk, government role and market risk.[6] In all three portfolios, each involving 23 energy options, fusion was ranked and prioritized almost dead last – 22nd out of 23 DOE energy programs.[7]

In light of DOE's low ranking (on the merits) of fusion in terms of funding priorities, the focus on tritium fueled tokamaks is inappropriate for a number of reasons:

> (1) Electric utilities have shown little interest in fusion reactors and the Electric Power Research Institute (EPRI) abandoned it's fusion research effort in 1983. In a lecture to the American Nuclear Society, W.C. Wolkenhauer of Washington Public Service System, stated that "...Clearly an apparent wide gulf exists between the goals and attitudes of the DOE's fusion program and the utility community. The utilities are simply planning for a different world than the program is planning for... Can you imagine trying to develop a new weapon or aerospace device without some accurate mission objectives specified by ultimate

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[5] "Renewable Energy and the National Energy Strategy," Hearing Before the Subcommittee on Investigations and Oversight of the Committee on Science, Space, and Technology, U.S. House of Representatives, One Hundred Second Congress, Second Session, April 30, 1992, p.28., p.A-6.

[6] Ibid, A-6, p.66.

[7] Ibid, pgs. 45-48.

> users of such devices? Yet that is just about the situation in fusion development."[8]
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> (2) The tokamak was originally designed as a plutonium breeder, not a power source. An Office of Technology Assessment report noted that "a [tokamak] fusion reactor's ability to breed fissionable materials such as uranium and plutonium could possibly increase the risk of nuclear weapons proliferation."[9]
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> (3) Large amounts of radioactive waste would be generated by tokamak reactors. Dr. Robert Hirsch, former Vice-president of the Electric Power Research Institute (EPRI), testified in 1993 before the Senate Committee on Energy and Natural Resources that "...DT [deuterium-tritium] tokamak fusion reactors using the best available steel in their construction will produce more radioactive waste than an ALWR [advanced light water reactor]."[10]
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> (4) Even if the current tokamak oriented program was able to design and build a fusion reactor, the price for the electricity generated would probably not be competitive with other energy resources. Dr. Lawrence Lidsky, Professor of Nuclear Engineering at the Massachusetts Institute of Technology has noted that "...It is hard to make an economically based argument for fusion. You can't justify it, especially as other sources of energy look better and better. We're going down a narrow channel. Eventually there will be nowhere to go. By focusing on the tokamak, we're committed to a technology that is a commercial non-starter. And shifting gears to another design down the road won't be easy."[11]

For these and related reasons, particularly the question of energy R&D priorities and costs, the Council believes that DOE should shift its focus away from tokamak technology and concentrate on basic plasma physics and alternative fuels, such as light helium. This is hardly a new idea. In 1980,

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[8] "Lecture by W.C. Wolkenhauer, Washington Public Service System", American Nuclear Society Meeting, 1976.

[9] "Starpower: The U.S. and International Quest for Fusion Energy", Office of Technology Assessment, OTA-E-338, October 1987, p.112.

[10] "Testimony of Dr. Robert L. Hirsch, Vice-President, Electric Power Research Institute", Before the Senate Committee on Energy and Natural Resources, May 6, 1993.

[11] "Nuclear Fusion", CQ Researcher, Vol. 3, January 22, 1993. p.52.

DOE's Energy Research Advisory Board recommended a strong program on helium-3 based fuels.[12] The U.S. Senate Energy and Natural Resources Committee passed a resolution in 1982 noting that "basic fission and fusion research has largely overlooked aneutronic [non-radioactive] nuclear alternatives using light elements [helium]...The Committee recommends the Department give higher priority to this non-radioactive and nonproliferatory nuclear potential."[13] Unfortunately, during the last twenty years the Department has not seriously investigated alternative fuels/aneutronic fusion. In 1990 the alternative concepts program was essentially terminated.[14]

The FEAC Report

The major theme of the FEAC report is continued promotion of tokamak technology in the U.S. and abroad with increased general support for basic plasma physics and alternative fusion concepts. Because of the continued focus on tokamaks, the FEAC report, in the Council's opinion, can not be considered a fundamental restructuring of the fusion program as outlined by the U.S. Congress in the FY 1996 Energy and Water Appropriations Bill.[15]

For example, FEAC proposes the three large fusion reactors in the U.S., the Tokamak Fusion Test Reactor, the DIII-D tokamak, and the Alcator C-Mod tokamak continue to operate through FY 1997, and DIII-D and Alcator would continue through 2001. The report states that The International Thermonuclear Experimental Reactor (ITER), a collaboration involving the U.S., the European Community, Japan, and the Russian Federation, is currently in the design phase but will apparently provide a "test bed" for tokamak physics.[16]

The FEAC report's discussion of alternative concepts is similarly oriented toward tokamak related technologies such as the spheromak tokamak and the spheromak, and there is a suggestion that "the science program carried out on alternative confinement concepts should be closely integrated with the tokamak program."[17] The FEAC report generally endorses expanding a program for basic plasma science and alternative concepts research and recommends that 5% of the fusion budget be allocated for fundamental plasma sciences in the outyears of the constant level of funding effort. This level of funding is inadequate to rejuvenate plasma science in any meaningful way.

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[12] "Review of Magnetic Fusion Program", J. Buchsbaum et al., DOE's Energy Research Advisory Board, 1980.

[13] U.S. Senate, Report 98-153, 98th Congress, p.104.

[14] "A Restructured Fusion Energy Sciences Program", A Report by The Fusion Energy Advisory Committee Submitted to Dr. Martha A. Krebs, Director, Office of Fusion Energy Research, U.S. Department of Energy, January 27, 1996, p.E-1.

[15] U.S. House of Representatives, Congressional Record, October 26, 1996, p. H 10955.

[16] See N.14, supra,p.24.

The Council makes the following recommendations for the Subcommittee's consideration:

(1) In general, the DOE should reduce its commitment to tokamak technology and emphasize basic plasma physics. DOE's top energy priority should be sustainable technologies such as energy efficiency and renewable energy resources.

(2) The Tokamak Fusion Test Reactor at Princeton University should cease operations in FY 1996 and any additional funding for FY 1997 should be restricted to termination costs. The operation of DIII-D beyond the current fiscal year should be financed by private industry, Similarly, an alternate source of funds should also be identified for the Alcator facility without relying on further taxpayer support.

(3) The Council believes that funding for a tokamak oriented ITER effort is not cost-effective. The U.S. should not spend any funds for the ITER project beyond contractual commitments presently in force through FY 1998 unless the project is re-oriented toward alternatives to tokamak technology. A thorough review of ITER, (including technological, socio-economic, and environmental criteria) should be undertaken along with a cost/benefit comparison with renewable alternatives.

(4) The Department should allocate at least 10 percent of the FY 1997 fusion budget for basic plasma science and another 10 percent for advanced fuels such as light helium. Part of this allocation should be used to convene an international conference on alternative concepts and fuels.

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[17] Ibid., p.22.

Chairman ROHRABACHER. Thank you, Mr. Adams. Mr. Bartlett has a speech that he is going to be late for unless we go right away to Mr. Bartlett.

So, we are going to pay him the courtesy of letting him be the first to question.

Mr. BARTLETT. I appreciate that very much. I still haven't found out how to be in two places at one time.

So, thank you. Thank you very much.

Dr. Krebs, it's good to have you on our Committee again. Each time you are here, I am reminded of your famous namesake and what a great name to have in any science area.

Dr. Krebs, a Britisher, who, as you may know, described first this big intermediary metabolism cycle that is common to so many life forms, variously called the citric acid cycle, the tricarboxylic acid cycle or, more commonly, the Krebs cycle. It's nice to have you with us again.

Mr. Schatz, you recommend that cutting down the TFTR—shutting that down immediately. The previous panel had made the point that we have a very large investment in this.

And, it didn't make any sense to them—and I'm kind of persuaded by their argument, it didn't make any sense to them to shut this down now when we still had a couple of years of opportunity to use that for what they considered some very important and useful basic research. Aren't you persuaded by that argument, that with this very large investment that we should extract out of it all that we can before we shut it down?

Mr. SCHATZ. I think the point that I made is that you can always stretch these out and say, "If we can just get a little more." I think what I am suggesting to this Committee is that really, in a sense, it's your decision.

We are making a suggestion that it be shut down now. If it's your decision, within the fiscal constraints that you face, that there is more to get out and you can do everything else you are going to do, then that's ultimately honestly your decision.

But, it's our view that the Subcommittee last year said this should be shut down. And, now, all of a sudden, it's another year, another two years, another three years.

And, there is really no certainty.

There are the other two tokamaks that are out there. It has been stated that they do some different things.

We are also suggesting that some of this money be put back into other investments. There is always a time to say, "That's it."

We may not have gotten everything we can. And, sometimes you can't get everything that you might want out of an investment by the federal government.

Mr. BARTLETT. But, we do have the difficult task here of weighing these alternatives in deciding when it's best for everyone concerned to cut and run. I appreciate your input.

Both Mr. Gavin—and I appreciate very much your testimony—and Mr. Adams spoke of an energy future. And, I gathered from some of the things you said, Mr. Gavin, that you are concerned about our energy future, that we need to be exploring other types of energy.

Is that a correct assessment?

Mr. **Gavin**. Yes. I am concerned about the matter of global stability because of the disparate demands for energy.

I think that if you look at the political situation in the Middle East today, North Africa is in a fundamentalist ferment. Who is to say to what degree the Gulf resources will be available to us? Can we afford to keep spending as much money as we do every year for petroleum?

I think that one of the things that I would like to comment on is that there has been today an interesting distinction made between research, science, applied science and so forth. Now, I speak as an engineer.

And, in my experience, the progress from science to product is not a linear progress. It isn't until you try to develop something that you discover some of the questions that need to be examined by the scientists.

So, it's a reiterative process. And, I think it's better to think of it as a continuum rather than as a series of separate steps.

And, I'm a little dismayed to hear these distinctions made today, because I think they are artificial.

Mr. **Bartlett**. In terms of developing the final product, you are exactly right. Having worked in both communities, I agree with you totally.

I was in the basic research community and then moved to the engineering community. And, so I had a foot in each of those.

Just in closing, and my time is about up, Mr. Adams spoke of renewables. I am a real enthusiast for renewables.

I have a place in the mountains in West Virginia. And, we have solar there. And, we have some wind power there, a wind machine there.

But, my concern is that as enthusiastic as I am for them—and I am concerned that we have not put enough emphasis on developing the renewables—it was a general assessment I thought a few years ago. And, I'm wondering if that has changed?

That if you are even—if you were quite optimistic about all of these renewables and assume success in every one of them, that when you add all of them up, you still come short of the absolutely enormous amounts of energy that we are now using from our fossil fuel supply. So, my enthusiasm for the renewables—and we ought to be putting more money there but that doesn't dim my enthusiasm for fusion, because in the long term we are only home free with fusion.

We are going to run out of gas and oil relatively soon, coal not long after that in terms of the stretch of the human race. And, after that, we are stuck then with the renewables and with fusion.

So, my enthusiasm for the renewables doesn't keep me from also being very supportive of fusion, because we can't do what we are now doing. Our lifestyle globally, and particularly in this country, would change dramatically if the only power we had in the future was from renewables.

Now, is that not still the general consensus?

Mr. **Adams**. Well, first of all, we agree that fusion should be investigated. Our concern is that we want relatively clean—that is, non-radioactive—and affordable energy.

And, we believe that there is a role for fusion.

So, I don't want to imply that we are saying that we are not supportive of fusion.

On the other hand, I also believe that we don't realize the potential for renewable energy to replace a lot, if not most, if not all, of the fossil fuel-based energy that we have now. We have just barely started exploring wind and solar.

The whole subject of hydrogen is something that we are just getting into. So, in some sense, I think the situation has changed from what might have been common knowledge four or five years ago, that we can't meet all of our energy needs with just renewables.

In fact, I think we can. It's a question of we have to move down that path.

And, that's why, like you, we would like to see more money for renewables. Our concern is that renewables goes up against tokamak-based fusion and keeps getting money cut. And, that is our concern.

But, we are not opposed to fusion being a part of a broader, sustainable energy path of budget. And, that's our concern.

Mr. BARTLETT. Thank you. And, thank you, Mr. Chairman.

Chairman ROHRABACHER. Thank you, Mr. Bartlett. The concern that many of us have had is echoed by Mr. Adams; and, that is that we are not just talking about spending money on a particular project when we talk about setting priorities. We are talking about draining resources away from other programs, not only financial resources but also human resources.

One of the first decisions I made as a member of this body was whether or not we were going to spend—and I think it was a proposal for $650 million to spend on whether or not we would be developing HDTV. And, the Japanese were way ahead of us. And, we had some television manufacturers here from the United States, and we were supposed to subsidize their effort to catch up with the Japanese.

And, that money would have gone—and I voted against it. And, that money would have gone, had it been spent, to developing HDTV base on analog technology.

And, not only would it have not been a good investment, it would have taken people who were involved in developing digital HDTV, which we now know is the way to go, and taken them into a counterproductive activity. It would have actually hurt America's development.

And, that's why we have to be very, very careful when we take, you know, millions and millions of dollars and put it towards one big project rather than letting things go in maybe a more diverse way towards solving a problem. And, with that said, Mr. Roemer would like to have his time now, because he also has a commitment.

Mr. Roemer.

Mr. ROEMER. Thank you, Mr. Chairman. I appreciate that.

First of all, thank you. We are delighted to have you, Dr. Krebs, before the Committee.

Your expertise and your presence here is always a significant help toward helping us make informed decisions on this Committee. So, thank you for coming up here this afternoon.

Mr. Adams, let me start with you just so that I make sure I understand your position. You are strongly in favor of wind and solar renewable resources.

Is that correct?

Mr. ADAMS. That is correct.

Mr. ROEMER. I hope that you are asked to testify when proposals are made to cut those programs in this next budget and that you will strongly fight against those different proposals.

Mr. ADAMS. I would be happy to do that.

Mr. ROEMER. Secondly, you are not for a eliminating the fusion program; you are simply for making it work better and reorganizing it and having it be one of many eggs in the basket of options for us to pursue in the future.

Is that correct?

Mr. ADAMS. That is correct.

Mr. ROEMER. Okay. Thank you. Mr. Gavin, the first panel of witnesses said that they wanted to support fusion R&D at about $275 or $265 or $300 or $320 million, that there was a difference there in terms of the funding levels between a world class organization and dedication toward fusion research, toward a viable U.S. program.

And, now we've heard the term from you to a third-rate program, I think you said in your testimony. What level do you think keeps us out of that third-rate designation?

And, what level of funding are you in favor of? And, finally, are you supportive of the construction phase of the ITER?

Mr. GAVIN. Well, let's answer in reverse. I——

Mr. ROEMER. That will probably help you give funding of——

Mr. GAVIN. I do not see how we are in a position to have an equal portion of the construction phase of ITER. I think that has been covered by some of the other people quite adequately, that we would try to find a participation that would fit our budget but would not commit us to equal support of the construction.

Not only do we not have the funds to do it, but I don't know of any mechanism where this government can make a 10- or 15-year commitment that will stick.

The second part of your question had to do with the funding level. I picked the $320 million resulting from the PCAST study, because I found on re-reading the study here recently that that was a very broad-based, carefully-assessed study.

And, I think that, frankly, they had more time to produce their result than the current FEAC Committee had. And, it was well done. It will bear reinspection.

And, as I said, I believe that that's a holding position, not a world-leadership position. I think at any of the levels below, say, $300 million you are talking about being third rate, because I look at the investments being made by the Europeans today and the Japanese today and there is no question but what they are proceeding at a rate that we cannot match at those lower funding levels.

Mr. ROEMER. Thank you, Mr. Gavin. Finally, Mr. Schatz, I am delighted to have you before the Committee.

You and I have worked together on a host of different proposals to work toward a balanced budget. And, I want to salute and commend your organization, Citizens Against Government Waste, for the very good job you do for the taxpayers of this country.

As you know, I voted for the Blue Dog budget that balances the budget in seven years, that still maintains investment in education and technology and R&D efforts in this country and have opposed such things as the B-2 and the space station and support cuts in the CIA. You have supported, in your testimony, about a $65 million cut in the fusion program.

You have not advocated, again, an elimination of this program. Is that correct?

Mr. SCHATZ. That is correct.

Mr. ROEMER. So, you do believe that fusion should be some kind of option for us, in DOE and as citizens of this country, to pursue some viable option that this may present in the future?

Mr. SCHATZ. Oh, absolutely. And, again, looking at the Grace Commission's report, which did examine this issue at some small length, they did say that there is certainly this potential.

And, again, it's a question of prioritizing and finding enough money and making some tough choices. But, certainly we support continuing and moving forward.

Mr. ROEMER. Thank you, Mr. Schatz. It's nice to see you again.

Mr. SCHATZ. Thank you.

Chairman ROHRABACHER. Ms. Johnson.

Ms. JOHNSON. I have a brief question. I indicated earlier that I wouldn't, but——

Chairman ROHRABACHER. Sure, go right ahead.

Ms. JOHNSON. I would like to ask Mr. Adams and Mr. Schatz, could you identify some of the private sources that some of the programs could access to fund some of the projects privately?

Mr. ADAMS. Well, with the General Atomics-run facility, obviously we would look towards them. I mean, we need to have some sort of industry cost-share here.

I mean, the point is that if these facilities are performing valuable research, that is one thing. The problem is I just don't think the taxpayers can continually support these for year after year.

So, for at least the San Diego facility, we would look at General Atomics itself to pick up some of this fund, because I mean this is a commercial entity that is operating a facility. And, it seems to make sense for them to be picking up the cost.

Ms. JOHNSON. Well, let me—you mean, a public/private partnership or the ceasing of federal funds altogether?

Mr. ADAMS. Well, we think both should be looked at. Perhaps it could be phased out over a period of years or cost-share could occur over several years.

We haven't really made a definitive thing one way or another. We would like to at least see it go in that direction.

And, ideally, at some point, we would like to see it completely supported by the commercial entity itself.

Mr. SCHATZ. I would echo that and also note that in the long run—and many questions have been raised about the commercial viability of tokamaks, that this could be 30 to 40 years down the road, $30 billion. I think it's our view that that's a huge expense for taxpayers. It's also—it may not be affordable in the private sector.

Certainly, it—we, again, are not opposed to this kind of research. But, given what we are looking at in terms of budgets and what we are looking at in terms of really other priorities—education. Choices have to be made.

We wish we could do everything. If we had a balanced budget, maybe we could.

But, again, this is a suggestion. I am not answering your question in whole, because I don't know. I could not identify specific companies or venture capitalists that might look at this and say, "This is a great idea."

But, we certainly will continue to look at it. And, if we have more suggestions, I will come back to you with them.

Ms. JOHNSON. I like the idea. It's just that I don't know where the source might be for any commercial industry to invest in that kind of research that might be 30 years or 40 years out.

Mr. SCHATZ. Right. Well, again, I think a good question is why should the taxpayers if the commercial industry isn't willing to do so or, at least, start looking at contributing more towards it.

And, maybe it's not total privatization. Maybe it is a public/private cooperative effort of some kind.

But, maybe they can answer that question, too.

Ms. JOHNSON. So, you are saying that—I mean, most of our research ultimately is commercialized; I mean, the results of our research, which the government has taken a major responsibility for in the past. And, all of us would like to shift responsibility.

But, I wonder if that is feasible.

Mr. SCHATZ. Sitting here right now, I don't think I have the answer to that question. But, I think it's worth examining and maybe asking the company itself.

Ms. JOHNSON. Sure.

Mr. GAVIN. May I have a word?

Ms. JOHNSON. Sure.

Mr. GAVIN. Having spent 40 years in industry, I think that there is a great delusion about cost-sharing. The way the tax laws are constructed and the way the stockmarket works, there is very little incentive for a company, unless it's privately held and not a public company, to make an investment that's 10 to 20 years before paying off.

It's very straightforward.

Ms. JOHNSON. Thank you.

Chairman ROHRABACHER. Thank you very much, Mr. Gavin, especially your last remark indicating how much we can solve by changing the tax structure to give companies incentives to do these things rather than relying on the government just to shovel money out the back of the truck.

Mr. Roemer, would you like——

Mr. ROEMER. Mr. Chairman, I would just make a unanimous consent request that the record be held open for two weeks so that we can have the opportunity to add additional material for the record.

Chairman ROHRABACHER. Without objection.

Mr. ROEMER. Thank you, Mr. Chairman.

Chairman ROHRABACHER. All right. And, Mr. Ehlers.

Mr. EHLERS. Thank you, Mr. Chairman. First of all, I will add approval to the comment by Mr. Gavin about the tax laws. I think, in many cases, a very effective way of funding research should be a substantial federal tax credit for money given for basic science and fundamental research.

I do apologize for having missed much of the panel. It's one of these horrendous days with four meetings going on simultaneously. And, I've been dashing thither and yon.

I did review much of written testimony. And, I appreciate your coming here and appreciate the comments that you have offered.

I only have one question. And, that's not even directly related to the issue here.

But, it's for Mr. Schatz and something that pertains to his concern about the future of energy research, as contained in your written testimony. And, I think you had some good thoughts there.

But, I was just wondering if anyone in your organization has taken a good look at energy futures—not just fusion but in general—and particularly the role that energy efficiency plays? I know that you have put a lot of effort into trying to improve government efficiency.

One of my concerns is that America, as a whole, and particularly American industry, has not picked up on energy efficiency the way they should. And, I am wondering if anyone in your organization has done any work in this and what ideas or advice you might have of ways that Congress could encourage greater energy efficiency in this nation without substantial expenditures of federal money?

Mr. SCHATZ. That's an excellent point, Congressman. And, again, I believe the Grace Commission did have some thoughts on that.

Of course, I am old enough to remember the lines in the 70s for gas and the other problems that were caused at that time.

Mr. EHLERS. You don't look that old.

Mr. SCHATZ. And being cold in 1977 when President Carter turned out the lights at the White House. But, in any event, this is a very, very important subject and one, as you point out, that should be looked at in a way that it does not cost taxpayers but, in fact, saves money.

I know there are many companies that will go into, say, a large building and will take, as their payment, any of the savings that occur as a result of reducing the energy consumed in those particular buildings. I don't know the extent to which the private companies have gone and done that.

Certainly, in the federal government buildings, if they have not, they should because there is certainly a great deal of savings to be made there as well. And, that is a direct savings to the taxpayers.

I recall at some point—I worked for Hamilton Fish for six years in the 1980s, early 80s to 1986, and he was on the Energy Subcommittee on the Science Committee. And, I recall some of these discussions coming up. But, I have really not followed it a lot since then.

But, I will be happy to look further. I work with other groups, so I would imagine Mr. Adams' group is interested in those same kinds of issues.

Mr. EHLERS. Yes. I would just like to encourage you both here to do whatever you can to pursue this, because the public has been lulled into a feeling that there is no energy crisis, no problem, we should have bigger cars, et cetera.

Mr. SCHATZ. We should put my grandmother in charge of this particular issue.

[Laughter.]

Mr. EHLERS. They remember.

Mr. GAVIN. Mr. Ehlers, I've had some direct experience in the matter of selling energy conservation. And, you can sell an investment in energy conservation if the payoff is within the—if the break even is within four to five years.

If it exceeds that, forget it. You can't sell it.

And, we've done that. We did it extensively. In fact, we did it to our own company where we saved something like 30 percent in the expenditure of energy.

But, if I were to go into any institution today and say, "I can save money by changing your air conditioning, your heating system or your insulation," it boils down to showing break even within five years or it's no sale.

Chairman ROHRABACHER. Is there something in the Tax Code that indicates five years? I think there is. Isn't there? You get a depreciation schedule.

Mr. GAVIN. I'm not sure of that.

Mr. EHLERS. It's largely time value of money. But, my frustration has been in cases where I could demonstrate payback periods of less than that, I could not persuade companies to do it.

They just assume it's some fuzzy-headed, liberal idea, that it won't really do anything. And, they disregard it.

So, I think we have to do much more in this nation to try to persuade that. Yes.

Mr. ADAMS. Two comments. One is that I agree with you completely. In fact, in California, where I come from, we had a very innovative program to replace lighting and fixtures and whatnot in schools and public buildings. And, the payback was on the order of two to three years.

It is true that when you talk about replacing heating and air-conditioning systems, you really have to have a 10-year or a 15-year payback period. And, it was difficult because of laws and taxes and whatnot to do that.

But, we were, at one time, trying to persuade the California Legislature to change it so that we could do that because, for instance, schools have some very antiquated heating and air conditioning systems and they are wasting so much energy and losing so much energy annually. And, the reason is they just have to completely replace their system and they can't do it, because the law doesn't allow them to do it.

So, I would also mention there is an organization, the American Council for Energy Efficient Economy here, based in Washington, D.C., whose whole focus is how energy efficiency can be improved in this country. It's one of the organizations that we work with.

And, I could get you information from them. They have a whole—very, very extensive on all levels from refrigerators to everything. So, it's something we are very much interested in.

Mr. EHLERS. I'm afraid I don't need the information. But, the American public does.

Chairman ROHRABACHER. Mr. Ehlers, thank you very much.

Mr. EHLERS. Thank you very much.

Chairman ROHRABACHER. And, I think that—we are called for a vote now. But, let's give Dr. Krebs a minute to summarize your Department's position, And, I will give you the final say here.

Dr. KREBS. Thank you, Mr. Chairman. I have been sitting here listening to these comments.

And, I would like to, first of all, say that I think Mr. Gavin states compellingly—and, in fact, I sat here before this Subcommittee I think last year with some of the same articulation of the problem that we see in the coming century, but he states the case for fusion compellingly.

And, I still believe, in fact, in the role that fusion can play in the next few years.

He also states compellingly what the opportunity for both U.S. science and U.S. industry could be in terms of an international market and the international need for fusion energy.

I also believe that he states compellingly the need not to distinguish too rigidly between basic and applied science and the contributions and the interactions. And, it is this statement that, for me, answers the question that Mr. Schatz raises as to whether or not this is a sensible investment and at what scale it ought to be made for the American people, even though Mr. Gavin and I disagree at least at this point on the size of the investment.

One of the things I've learned in the last year is what it means to be in the Executive Branch.

However, I would like to say that I do not believe—I disagree with Mr. Gavin when he says that we will be a third rate in fusion. We may, indeed, not participate at the level that we would have liked to a few years ago, but we will be first rate in science.

And, the way we will do that is by relying on external peer review, as we have in our many other science programs. And, that is how we intend to go forward in making the balance between plasma science, alternatives and improvements in the tokamak technology.

And, just in final, relative to Mr. Adams' comments, I believe that he sees a competition that this Administration does not, in the sense that we look at fusion, renewables and energy efficiency as both a—well, for the long term as a portfolio of options that are important to the people of this country. And, in the 1997 budget, we will make a proposal that does not trade them off against each other.

Chairman ROHRABACHER. Thank you very much, Dr. Krebs. Questions will be submitted to you in writing with some time to answer those questions for the record.

Thank you very much. This hearing is adjourned.

[Whereupon, the hearing was adjourned at 1:05 p.m.]

[The following material was received for the record:]

OPENING STATEMENT
CHAIRMAN DANA ROHRABACHER
Fusion Energy Hearing
March 7, 1996

40 years. $14 billion. Using constant 1995 dollars, that is the investment this country has made in the quest to develop fusion energy as a practical source of power.

A year ago we were looking at a fusion program outline that would have nearly doubled the annual cost with a timeline well into the next century. That game plan ignored budget realities and set no priorities. Following it would have meant another 40 years and another $14 billion, at least. I think even its strongest supporters would agree that this unrealistic approach was more harmful than helpful to the development of fusion energy in America.

The Fiscal Year 1996 Energy and Water Appropriation signed into law by the President wisely called for a restructured program more in line with budget realities.

To its credit, the Fusion Energy Advisory Committee, charged with this responsibility by the Department of Energy, has done just that.

In its report, the Committee states, "The underlying theme of the restructuring is to redirect the program away from the expensive development path leading to a fusion power plant, to focus on the less costly critical basic science and technology foundations."

This is commendable.

Yet the policy recommendations outlined in the report do not seem to follow this direction.

The Committee's proposal would have us spend the bulk of the fusion budget on a variety of projects centered around the tokomak concept. For example, the committee's proposals call for running all three Tokomak facilities in FY '97, although TFTR was originally scheduled for termination last fall!

In a letter to me dated March 4, Professor Lawrence Lidsky of MIT, who worked on plasma physics and fusion technology for over 20 years, said the fusion program does not deserve support as long as it continues its "relentless focus" on the development of a Tokomak power plant, which

Professor Lidsky describes as "large, expensive, unforgiving of error, and intensely radioactive."

We have also heard that this "relentless focus" has shoved aside a serious consideration of alternatives to the Tokomak concept.

Our witnesses today, representing a large spectrum of views, will allow us to determine whether these criticisms are justified.

All are agreed the Fusion Energy program is at a critical juncture. I believe this hearing will be helpful in giving the members of the Subcommittee guidance they need to make the budget choices for FY '97.

# # #

Opening Statement
The Honorable Eddie Bernice Johnson

Eddie Bernice Johnson

Energy and Environment Hearing
Fusion Energy

3/7/96

I thank you for recognizing me and for calling this hearing this morning, Mr. Chairman. With today's hearing we again begin the process of determining the proper authorization levels for fusion program in the federal budget.

As a supporter of research, I was less than pleased with the 33 percent cut in fusion programs this Committee played a part in during the FY 1996 appropriations process. We enter today's hearing with two recently completed studies to assist us in making the proper determinations about funding levels. Those are a June 1995 study by the President's Council of Advisors for Science and Technology and a January 1996 study by the Fusion Energy Advisory Committee (FEAC). The level recommended by these two panels ranged from a low of at least $250 million by FEAC and a high of $325 million recommended by the President's Advisors.

The United States needs to maintain its commitment to continuing fusion research. This enormous energy source can be of high value in the future. I hope today's witnesses can enlighten this Subcommittee as to the minimum amount necessary to maintain a viable and effective fusion research program for the United States. Additionally, I hope our witnesses can comment on the value of international cooperative efforts in fusion research, such as the International Thermonuclear Experimental Reactor (ITER). I look forward to today's testimony.

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Statement

For the Record

of

Marvin McKoy
Chairman
Energy Policy Committee

Institute of Electrical and Electronics Engineers -
United States Activities

before the

House Committee on Science
Subcommittee on Energy and the Environment

March 7, 1996

Mr. Chairman and members of the Committee, my name is Marvin McKoy. I am Chairman of the Energy Policy Committee of the Institute of Electrical and Electronics Engineers - United States Activities (IEEE-USA).

The IEEE is a transnational professional society whose 320,000 members live and work in more than 130 countries throughout the world. IEEE's United States Activities promotes the technology policy and professional career interests of IEEE's 220,000 U.S. members.

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APPENDIX I

Reliable and affordable electrical power is essential for the United States to sustain and increase its productivity and economic competitiveness and to support a high quality of life for its population. Our overall energy goals should foster the development and deployment of energy production and conversion technologies that will increase the efficiency of U.S. energy utilization, improve environmental quality and support U.S. competitiveness. Environmental impacts force us to address the need for alternative energy options. Sources worthy of exploration include advanced fossil fuel systems, solar and renewable energy systems, nuclear fission systems and thermonuclear fusion systems.

IEEE-USA has been alarmed by the particularly severe cutbacks in the budget for energy research and development, particularly in areas that meet the House Science Committee's Criteria for high-priority R&D. The Science Committee stated that "federal R&D should be focused on long-term, non-commercial research and development, with potential for great scientific discovery, ... revolutionary new ideas and pioneering capabilities that make possible the impossible ..." Fusion energy science research, the topic of this hearing, meets all these criteria.

First, fusion is long-term non-commercial R&D. Resolution of the scientific and technical challenges inherent in fusion demands solutions to scientific issues and innovation in concept improvement, burning plasmas, and low activation materials as described in the recent report of the Department of Energy's Fusion Energy Advisory Committee. The long time scale for these developments precludes private section investment.

Second, fusion is a field of science with the potential for great scientific discoveries and revolutionary ideas in the areas of plasma transport and turbulence, plasma stability, heating and current drive, power and particle handling, and plasma instrumentation and control. The production of the power of the stars as a source of energy on Earth would certainly qualify as a pioneering accomplishment, "making possible the impossible."

Recent scientific accomplishments in the U.S. magnetic fusion program demonstrate the strong rate of progress in fusion energy science:

- the discovery of very promising regimes of enhanced performance in the U.S.'s Tokamak Fusion Test Reactor (TFTR) and DIII-D; and

- the production of more than 10 million watts of fusion power in the U.S.'s Tokamak Fusion Test Reactor (TFTR);

A similar strong rate of scientific progress has been achieved in the U.S. inertial fusion energy program:

- the National Ignition Facility (NIF) is in the Title-I design phase. NIF is targeted at demonstrating ignition and burn in Inertial Confinement Fusion (ICF) targets, which is a key milestone in developing ICF as an energy source.
- Inertial Confinement Fusion target physics experiments and modeling for indirect (radiation) drive continue to increase confidence in achieving ignition on the National Ignition Facility.
- the upgrade project for the OMEGA glass laser at the University of Rochester was completed.
- demonstration that a driver energy of 5 to 10 MJ is adequate to achieve fusion yield many times larger than the required driver energy.

These advances in the U.S. fusion energy science program are impressive and demonstrate the scientific value of the program and the ability of the progress to meet the long-term energy goals. To be productive and to fulfill its role as an international partner, the U.S. fusion program must support a balance of domestic and international programs to remain among the world's leaders.

The fusion program should include an appropriate balance of burning plasma studies by participation in programs such as the ITER EDA, tokamak concept improvement as in the current base program, inertial fusion energy such as in the National Ignition Facility, alternate concepts, basic plasma studies and development of enabling plasma technologies. U.S. industry should be involved whenever appropriate so that it will have the skills necessary to compete in the international market for providing fusion reactors in the future.

It is regrettable that the U.S. government has chosen to relinquish leadership of the fusion program to Japan and Europe. While other developed and developing nations are increasing their level of effort, the U.S. has chosen to make significant reductions. We feel that the U.S. should be a producer of fusion power systems in the future - not a customer for foreign systems. To permit the U.S. to maintain some capability and to remain among the world participants, the U.S. government must invest in fusion research aimed at timely development of the scientific basis for fusion as an attractive power source for base load electrical power generation.

We thank you for your past support, we recommend that you invest in fusion consistent with the Science Committee's criteria for federal investment in R&D. We in IEEE-USA stand ready to provide technical information to assist you in your role.

Thank you.

DEPARTMENT OF NUCLEAR ENGINEERING

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

77 Massachusetts Avenue Cambridge, Massachusetts 02139-4307

Room: (617) 25

38-174 3-3808

March 4, 1996

The Honorable Dana Rohrabacher, Chair
Energy and Environment Subcommittee
House Committee on Science
Suite 2320 Rayburn House Office Building
Washington, DC 20515-6301

Dear Congressman Rohrabacher:

I regret that my teaching schedule prevents me from appearing in person before your committee. This letter, and its attachment, will have to be my only contribution to your hearing, which I believe is particularly important because we are running out of time to save the fusion program from complete collapse.

Many witnesses before the committee will, no doubt, argue that the waning support for the fusion program is a sign that Congress is not capable of understanding complex technology, or that the country is no longer willing to make long-term commitments to anything. I do not agree. I think the fusion program, *in its present form*, is not worthy of support. In fact, I believe that the current program is probably doing more harm than good by diverting support from research areas that could conceivably make far better use of the potential applications of plasma science and technology.

I will not use this opportunity to make detailed technical arguments; others will do that. I will, instead, recount a simple tale that I think has relevance and that points to a course of action.

In 1972, I developed a new course: MIT 22.63: Fusion Reactor Design. Although there had been significant attention given to the subject before this time, I believe this was one of the first academic offerings with so explicit and so ambitious a goal. The culmination of the course each year was the detailed analysis of a conceptual fusion reactor design. I was rather disappointed when the analysis of the first year's design showed that a fusion reactor would be large, complex, expensive, and highly radioactive. The next year, you can be sure that I proposed a very different concept. Unfortunately, we came to the same conclusion. I repeated this process twice more, with the same conclusion each time. I was willing to believe that I had chosen a particularly poor design the first or even the second time but, after four tries, I could no longer deny the truth. Any D-T based fusion reactor, by the laws of nature, would necessarily be large, complex, expensive, and highly radioactive.

I corroborated this disappointing conclusion in detail in a study funded by the National Science Foundation in 1982. (The Department of Energy was not at all interested in the topic.) The conclusion of the NSF study was that although a D-T fusion reactor might be a physical and technical possibility, it was certain to be a commercial disaster. These results were also presented in a widely circulated popularized version* whose conclusion "if we build a fusion reactor, nobody will want it," has never been refuted. In fact, more and more concurring articles and statements are appearing every year.

The greatest problem with the fusion program has been its relentless, ever-sharpening focus on the development of a deuterium-tritium fueled tokamak power plant. Originally promised for demonstration by the turn of the century, the demonstration plant is now acknowledged to be at least half a century in the future. Although this world-class "slippage" has done great damage to the credibility of the program, matters could have been even worse. For, although the quest for the D-T reactor has wounded the program, attainment of the goal would surely have killed it. A D-T fueled reactor would clearly be seen to be large, expensive, unforgiving of error, and intensely radioactive. It would also be impossible to ignore that it would be a prodigious source of untraceable weapons grade plutonium.

For those who doubt that the successful attainment of an ill-chosen goal can be the death knell of a program, one need only point to the SST, which crippled the European aircraft industry and to Phoenix and Monju, the breeder reactors which are the greatest disasters ever to befall the French and Japanese nuclear programs. It was clear, long before these projects were completed, that the assumptions that motivated the original program plans were invalid. However, institutional inertia, and unwillingness to admit to anything less than perfect foresight, kept these projects moving forward until they were "cheaper to complete than to cancel." This turned out to be a false economy; the completed projects now bar the path to development along much more interesting paths that have come to light since their completion.

The fusion program is clearly in trouble. It is supported only by those with a vested interest and those who are indirectly supported by it. No commercial entity invests in fusion and the rest of the scientific community ignores it. The financial and intellectual arguments for funding are growing increasingly unconvincing and the funding history reflects this trend. Although there have been studies and panels and assessments and "redirections" for the last 15 years, the trend continues.

The key to understanding the problem with the apparently different fusion reactor designs was the identification of the common factor. What lessons can we draw from the incipient failure of the fusion program itself? What is the common factor? Nothing less than the DOE management team and its insistence that the program set in place over 20 years ago is the one and only possible path to success. No program has been more closely micromanaged from Washington and few programs have missed their goals as widely. This may be for the best; few goals have been as poorly chosen.

Criticism is worse than useless without suggestions for improvement. The suggestions below are not made casually. Although I have been concerned with other matters for nearly a decade, I spent twice that length of time deeply immersed in the world of plasma science and technology and I am still a believer in its potential value. I have followed developments with great interest and passed many hours with ex-colleagues from around the world discussing their concerns. The following recommendations are based on those discussions.

First, I think we should withdraw from the ITER program as rapidly and gracefully as possible consistent with the promises we have made to our international collaborators. ITER is not just a waste of money; every step down the ITER path is a step we will have to retrace.

Second, I think we must broaden the scientific and application areas of the program, with far greater emphasis on external peer review as monitor of merit. Plasma science and technology is a rich field, with enormous potential payoffs in many areas in addition to electricity generation. Paradoxically, the more we widen our search and the better we understand the field in its totality, the more likely we are to achieve the sharply focused goal of economical electricity generation. Scientific merit and near-term applications must be given priority. These will more than pay for themselves and, with luck, we will eventually see our way clear to the ultimate, goal of economical power generation.

Third, I think attention must be given to the management of the program. It is doubtful that the necessary changes in management philosophy and style are possible without sweeping change in management personnel. I do not believe that the DOE is capable of effecting such change internally or with the aid of its long-term advisory panels. The fusion program has systematically excluded those researchers, advisors, and program managers whose interests were broader than those of the current managers and advisors. I believe it would be good policy to condition continued funding on a demonstrated infusion of new ideas and new blood.

Sincerely,

Lawrence M. Lidsky
Professor of Nuclear Engineering

LML:

*Lawrence M. Lidsky, "The Trouble with Fusion," Technology Review. October 1983.

The Trouble with Fusion

BY LAWRENCE M. LIDSKY

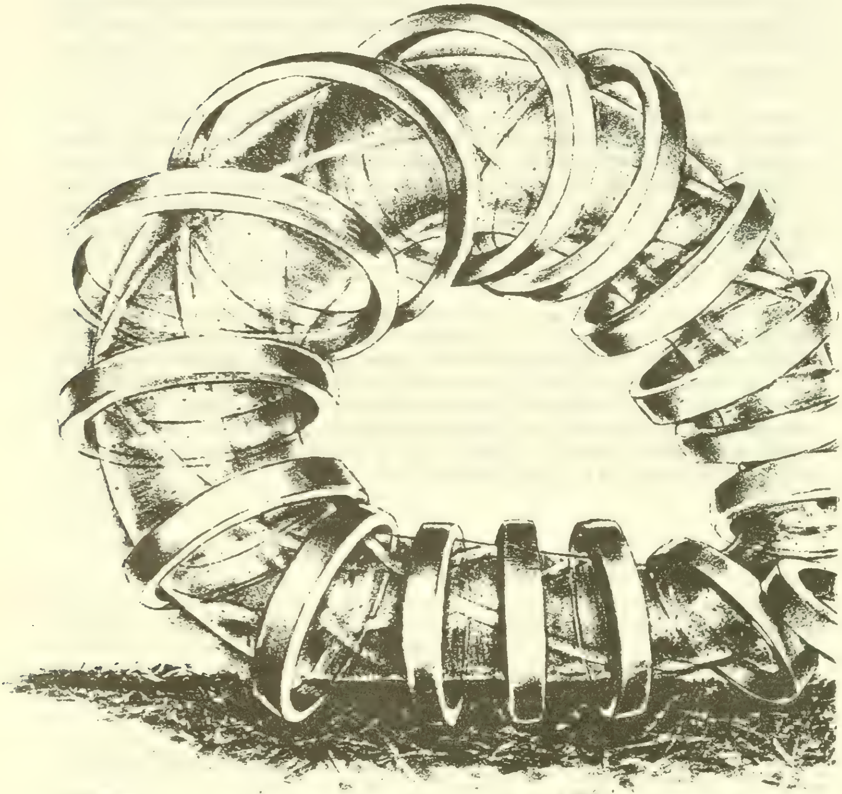
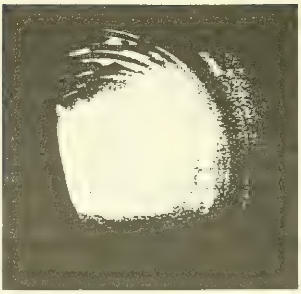


ILLUSTRATION: JANE BIANCO

THE technically advanced nations of the world will spend over $1 billion this year in the quest for controlled thermonuclear fusion power. This program has been sustained for 30 years with steadily mounting commitments of money and the dedication of an international group of scientists and engineers. Our knowledge of the related physics has grown enormously in the effort. Now the solution of the scientific problem appears to be almost within our grasp, and many assume that with it will come that technological Holy Grail: virtually unlimited, environmentally safe energy. But that outcome is unlikely. Instead, the costly fusion reactor is in danger of joining the ranks of other technical "triumphs" such as the zeppelin, the supersonic transport, and the fission breeder reactor that turned out to be unwanted and unused.

The dominating goal of the fusion program is to produce a reactor fueled by deuterium and tritium, isotopes of hydrogen containing one and two extra neutrons. This choice of fuel greatly eases the problem of achieving an energy-producing fusion reac-

Long touted as an inexhaustible energy source for the next century, fusion as it is now being developed will almost certainly be too expensive and unreliable for commercial use.



Scientists routinely fill vacuums with ionized gases, or plasmas, at temperatures of tens of millions of degrees Centigrade (above). To achieve a sustained fusion reaction, scientists at the Princeton Plasma Physics Laboratory plan to heat plasma inside this vacuum vessel (right) to temperatures 20 million degrees hotter than have so far been achieved.

tion, but the choice also has features that make it far more difficult to turn that energy source into a useful power plant. The most serious difficulty concerns the very high energy neutrons released in the deuterium-tritium (D-T) reaction. These uncharged nuclear particles damage the reactor structure and make it radioactive. A chain of undesirable effects ensures that any reactor employing D-T fusion will be a large, complex, expensive, and unreliable source of power. That is hardly preferable to present-day fission reactors, much less the improved fission reactors that are almost sure to come.

When these drawbacks become more widely realized, disillusionment with the existing fusion program will weaken the prospects for other fusion programs, no matter how wisely redirected, for decades to come. But such a result isn't necessary. The public has shown that it is enlightened enough to support long-range scientific research without a clearly defined near-term goal; witness the support for expensive research on high-energy physics. Furthermore, other nuclear reactions such as the fusion of protons with lithium or boron produce either fewer neutrons or none at all. A reactor based on these fuels would be far preferable to existing fission reactors.

Of course, we do not know how to build a reactor to ignite such "advanced" fuels. Indeed, we know that neutron-free reactions cannot be ignited in the magnetic bottles developed for D-T and, unfortunately, little of the physics painstakingly developed for D-T fusion will apply. There is no clear path for an alternative scheme, and not coincidentally almost no support. As a result, only a few researchers are at work in the field. But it is clear that if we can build a reactor employing neutron-free fuels, we can avoid the enormous, probably insurmountable, problems posed by deuterium and tritium.

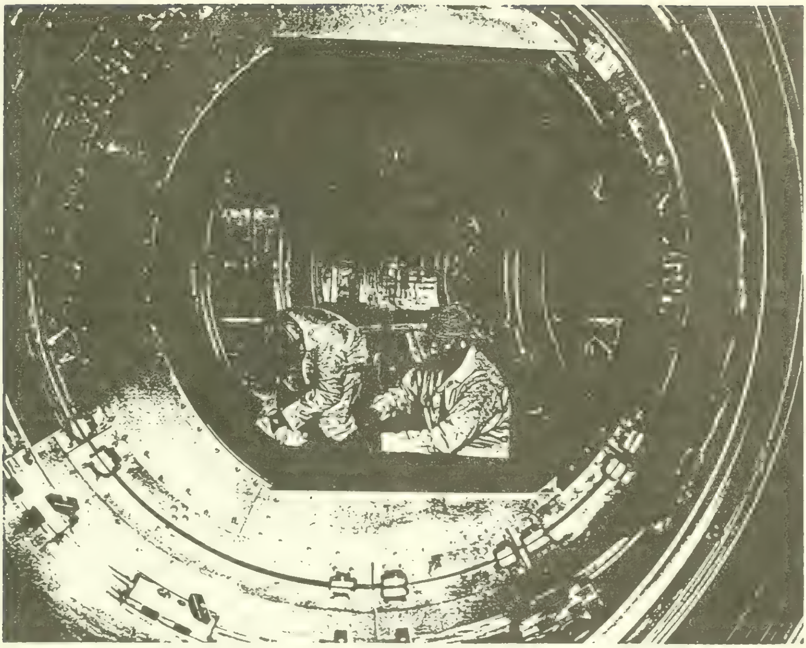
How could highly motivated and intelligent people get themselves into such a difficult situation? A fundamental reason concerns the difference between scientists' and engineers' view of what it means to solve a problem. Although they are usually able to agree on the definition of a "good problem," scientists and engineers often have different perspectives as to what constitutes a "good answer."

Good problems challenge our abilities to the limit but ultimately are solvable—that is, they are not so difficult that the time spent is wasted. In both science and engineering, the greatest satisfaction accrues to those solving a problem first, even though "better" (simpler or more complete) answers are often found later. In science, such answers can coexist peacefully and are usually mutually illuminating. However, engineering answers must meet economic and social demands from the start, and fundamentally different answers rarely coexist for long.

Fusion is a textbook example of a good problem for both scientists and engineers. Many regard it as the hardest scientific and technical problem ever tackled, yet it is nonetheless yielding to our efforts. We have made substantial scientific progress, and the advances in fusion-system engineering have been astounding. We have developed superconducting magnets that dwarf ordinary laboratory magnets. Today's particle beams are nearly a million times more powerful than those available at the beginning of the program. We routinely fill huge devices with ionized gases at temperatures of tens of millions of degrees and use lasers to measure their properties. The fusion program has stretched our abilities to the utmost, and we have responded.

The fusion program was, from its inception, dominated by scientists. In the best tradition of science, we chose the most promising target—D-T fusion—out of the dauntingly complex areas of thermonuclear physics, and we concentrated on it. We may well achieve that goal, which would be a scientific triumph. But the scientific goal turns out to be an engineering albatross. From the engineering point of view, we should have started from the answer and worked backward.

The second reason why intelligent and motivated people were led astray in fusion research is common to government programs that must compete annually for funds. There is a strong temptation to choose a near-term answer over a more rational long-term answer, even though this choice precludes reaching the



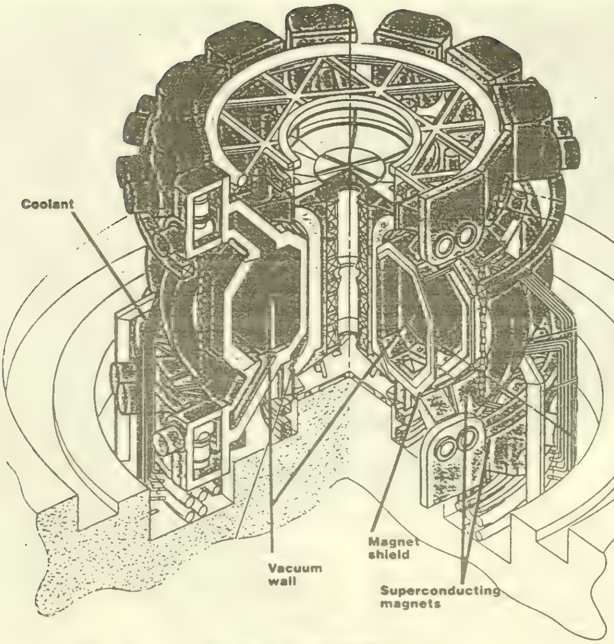
ultimate goal. The alternative would be the much more difficult task of developing support for a long-range program through persuasion and education. There is a related disinclination to adjust established plans, even if perceptions change. Indeed, it is considered dangerous even to admit uncertainty in a highly visible public program. Once established, an explicit goal, such as generating commercially competitive electricity from D-T fusion, is not easy to change.

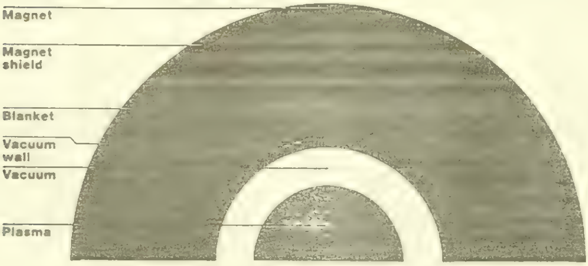
As a result, the Office of Fusion Energy of the U.S. Department of Energy has promised that it will, early in the next century, demonstrate the production of large amounts of power via D-T fusion. Producing net power from fusion is a valid scientific goal, but generating electricity commercially is an engineering problem. The requirement is to develop a power source significantly better than those that exist today, and D-T fusion cannot provide that solution. Even if the fusion program produces a reactor, no one will want it.

The Science of Fission and Fusion

Fusion and fission power both have their roots in nature's tendency to favor the nuclear moderate: the elements of intermediate weight are energetically preferred—that is, the elementary particles forming the nucelus are more tightly bound. As a result, energy can be released either when heavy nuclei are split (fission) or light nuclei are joined (fusion). Fission is far easier to achieve than fusion. Several atoms with heavy nuclei, such as uranium-235 and plutonium-239, are on the verge of splitting spontaneously; adding a single nuclear particle causes instantaneous fission. The nucleus splits into smaller fragments, releasing energy and several neutrons. These neutrons, because they are electrically neutral, can easily penetrate the electric barriers surrounding uranium and plutonium nuclei to cause additional fissions. This, of course, is the so-called "chain reaction."

PHOTOGRAPHS PRINCETON PLASMA PHYSICS LABORATORY





The schematic cross-section of proposed fusion reactors (bottom) has remained essentially unchanged from one proposed in a 1961 textbook. In the most likely scheme, called the "tokamak" (top), the tubular reactor is curved to form a torus (or doughnut).

Temperatures in the plasma where fusion takes place would approach 150,000,000° C. The inner surface of the vacuum (or first) wall (pink) encircling the plasma will be subjected to intense heat and bombardment by damaging neutrons from the reaction. The "blanket" containing lithium, outside the first wall, absorbs these neutrons to "breed" tritium for fuel. The engineering will be complicated by the fact that lithium reacts explosively with air and water.

On the reactor's exterior, the superconducting magnets that contain the plasma must be cooled almost to absolute zero. Hence the shielding to protect them from the extreme heat.

Despite the potential for problems in such a reactor, hands-on repair will be impossible because of radioactivity. All in all, the proposed fusion reactor would be a large, complex, and unreliable way of turning water into steam.

DIAGRAMS: OMNIGRAPHICS

Temperatures within a fusion reactor will range from the highest produced on earth to almost the lowest possible.

The problems with fission almost all stem from the smaller fragments of the original nucleus. We have no control over which of the hundreds of different fission products are formed, and, unfortunately, many are noxious, radioactive, toxic, or corrosive. These fission products are primarily responsible for the problems of reactor safety, including waste disposal and even the possibility of a meltdown.

Although fusion is conceptually simpler than fission, it is technically much more demanding. The root of the problem is that there is apparently no equivalent of the fission reaction that is induced by uncharged neutrons. All the nuclei that must be brought together for fusion are positively charged and, therefore, repel each another. This repulsive force between nuclei increases rapidly with increased atomic charge and becomes prohibitive for even moderately large atoms. Thus, it appears that fusion fuels must be chosen from among the lightest elements—hydrogen, helium, lithium, beryllium, and boron. But despite the relatively small number of light elements, more than 100 fusion reactions are possible.

Common to all is the fact that the reacting particles must be raised to very high energies (that is, must be very hot) to overcome their mutual electrical repulsion and approach close enough to fuse. Even at these very high energies, the particles are much more likely to bounce off each other at random angles—to "scatter"—than to fuse. Energy is conducted out of the system in this process. Thus, energy must be used to ignite fusion and to replace the energy continuously lost by the hot fuel. Obviously, the energy produced by the reaction must exceed the required input if the reactor is to be of any use.

But merely producing a net positive power output is not enough; achieving a high enough power *density* is also crucial. Power density refers to the rate of energy production per unit of reactor volume. Fusion will almost certainly have a lower power density than fission and therefore will require a larger plant to produce the same output. Suppose a fusion plant had to be ten times as big—and therefore likely ten times as costly—as a present-day fission plant to produce the same amount of power. Given the already intolerable costs of building fission plants, that would hardly be economically feasible. These issues of producing net energy and achieving a high enough power density are the dominant themes of fusion.

How Fusion Fuels Work

The choice of deuterium and tritium as fuels early in the fusion program evolved quite naturally. Deuterium is a nonradioactive isotope of hydrogen that, as mentioned, has one extra neutron in the nucleus. In nature approximately 1 out of every 6,500 hydrogen atoms is deuterium. Thus, it is abundant—after all, there is a lot of hydrogen in seawater—and separating it from ordinary hydrogen is straightforward because of the substantial disparity in the masses.

The first reaction seriously considered for fusion power plants was simply the self-fusion of deuterium—the D-D reaction. Deuterium reacts with itself to produce either helium-3, a stable but extremely rare isotope of helium, or tritium, the triply heavy isotope of hydrogen with two extra neutrons in the nucleus. These reaction products can themselves react with deuterium to produce even more energy than comes from the D-D reaction itself. Thus, a deuterium-fueled fusion reactor could, and almost certainly would, recycle and burn both the tritium and helium-3 in the so-called D-T and D-He^3 reactions.

Calculating the energy available from this complex series of reactions is the first problem assigned to students in my introductory course in controlled fusion at M.I.T. If they do their work properly, the students find out that the energy released by fusing the deuterium in one cubic meter of seawater equals that released by burning 2,000 barrels of crude oil. Every single cubic kilometer of ocean water therefore contains as much energy as the world's entire known oil reserves, and there are more than a billion cubic kilometers of water in the oceans. This astounding finding—in effect, an inexhaustible source of energy—shows why tens of billions of dollars have been spent and hundreds of scientists have devoted their entire careers seeking to tap this extraordinary energy source.

Unfortunately, making D-D reactions occur is extraordinarily hard, but there is an alternative. The tritium by-product that would be recycled in the D-D reactor is a far better fuel when mixed with deuterium than is deuterium itself. Not only is more energy released, but the combination of deuterium and tritium is 100 times more reactive than a simple mixture of deuterium. In other words, in similarly engineered reactors, a system fueled with deuterium and tritium will produce at least 100 times as much energy as one fueled by deuterium alone. Thus, as soon as scientists realized how difficult fusion was to achieve, they almost unanimously agreed that developing the D-T reactor should be the first goal of the fusion program. This scientific goal was well justified, and no one seriously questioned it as an engineering goal at the time.

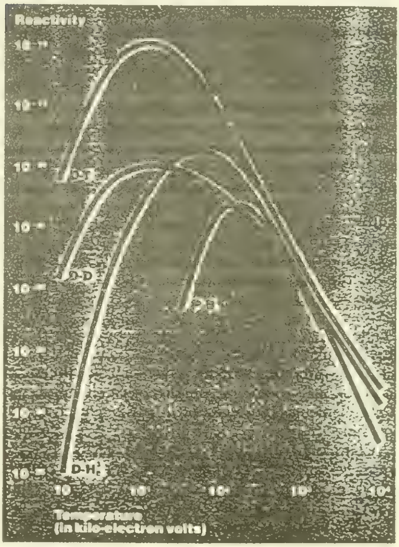
One of the first issues posed by the D-T fusion reaction was how to supply sufficient tritium. Tritium is radioactive, with a relatively short half-life of 12.4 years, and therefore it exists only in minute quantities in nature. Luckily, the neutron emitted in D-T fusion can react with an isotope of lithium to produce tritium and even release additional energy in the process. Though nothing compares with the vast store of deuterium in seawater, the world's lithium resources are enough for several thousand years of energy production. The lithium-neutron reaction resolves the tritium-supply problem. However, it introduces additional engineering difficulties.

Fusion Reactors: Large and Complex

The severity of the technical problems associated with the D-T reaction was not fully understood in the early years of the fusion program. But these difficulties have gradually been revealed by the extraordinarily detailed series of conceptual reactor designs produced under Department of Energy (DOE) funding over the last decade. The object of these studies is to describe a plausible fusion reactor based on the underlying physics and reasonable extrapolations of the technology. Of course, no one can be certain exactly what a D-T fusion reactor will look like. Nevertheless, several difficult questions that might seem to depend on this knowledge can already be answered. In particular: will a fusion reactor be simpler or more complex, cheaper or more expensive, safer or more dangerous, than a fission reactor? The answers depend only on the broad outlines of future reactors.

The main fusion reaction will take place in a gaslike plasma in which deuterium and tritium atoms are so energetic—so hot—that the nuclei have lost their electrons. The temperature of this gas will probably exceed 150,000,000° C. This plasma cannot be contained by physical walls, not only because no material could withstand the heat, but also because walls would contaminate the plasma. Instead, the plasma will be bottled within a vacuum by magnetic forces.

The fusion of deuterium (D) with tritium (T) is 100 to 1,000 times more reactive than the fusion of combinations involving helium-3 (He^3), protons (p), or boron-11 (B^{11}). In other words, a D-T-based power plant would yield 100 to 1,000 times more energy than an identical plant using the other fuels. That is why almost all research has focused on D-T fusion. However, the energetic neutrons it releases would damage and induce radioactivity in the reactor structure.

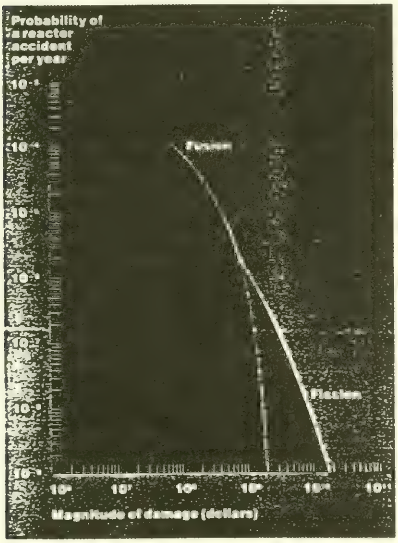


Four-fifths of the energy from the D-T reaction is released in the form of fast-moving neutrons. These neutrons are 15 to 30 times more energetic than those released in fission reactions. The first wall surrounding the plasma and vacuum region will take the brunt of both the neutron bombardment and the electromagnetic radiation from the hot plasma. This first wall is expected to be made of stainless steel or, better, one of the refractory metals such as molybdenum or vanadium that retain their strength at very high temperatures.

In colliding with this wall, the neutrons will give up some of their energy as heat. This heat must be removed by rapidly circulating coolant to prevent the wall from melting. After being piped out of the reactor, the heated coolant is used to produce steam and generate electricity.

Many of the collisions between neutrons and atoms in the first wall actually knock the atoms form-

The worst possible accident for a fusion reactor would destroy only the power plant itself—a minor hazard compared with the possibility of a meltdown in a fission reactor. However, a fusion reactor would be far more complex and prone to minor accidents. Since the fusion reactor would be too radioactive for hands-on repair, any accident could pose grave financial consequences for utilities. (The general shapes in the diagram are correct; however, the actual numerical values are uncertain and should not be taken literally.)



ing the metal out of their original positions. Each atom in the first wall will, on average, be dislodged from its lattice position about 30 times per year. Obviously, this causes the structure of the metal to deteriorate.

A few of the neutrons colliding with atoms in the first wall will have the beneficial effect of dislodging some neutrons from the atomic nuclei. These dislodged neutrons, plus the original ones generated by the fusion, pass through the wall and into the so-called "blanket," which contains lithium in some form. Here, the bulk of their energy is used to produce heat, which also is used to create steam for generating electricity, and eventually the neutrons are absorbed by the lithium to "breed" tritium.

Lithium itself poses serious engineering problems. It is an extremely reactive chemical: it burns violently when it comes in contact with either air or water and is even capable of undergoing combustion with the water contained in concrete. The lithium may be either in liquid form or in a solid compound. Liquid lithium blankets produce substantially more tritium and allow it to be more easily removed. However, the need to handle large amounts of this metal in liquid form leads to technical complexity and poses safety hazards.

The tritium-breeding region has other engineering requirements. It must be designed in such a way that the structural materials, as contrasted with the actual lithium, capture a minimum of neutrons. Also, the operating temperature must be high enough so that the coolant, when piped outside the reactor, can generate steam efficiently.

Outside the blanket, powerful magnets must provide the magnetic fields to contain the plasma. These fields will exert enormous forces on the magnets themselves, equivalent to pressures of hundreds of atmospheres. If made from copper wire, these magnets would consume more power than produced by the reactor, so they will have to be superconducting. Superconducting magnets, cooled by liquid helium to within a few degrees of absolute zero, will be extremely sensitive to heat and radiation damage. Thus, they must be effectively shielded from the heat and radiation of the plasma and blanket.

Temperatures within the fusion reactor will range from the highest produced on earth (within the plasma) to practically the lowest possible (within the magnets). The entire structure will be bombarded with neutrons that induce radiation and cause serious damage to materials. Problems associated with the inflammable lithium must be managed. Advanced materials will have to endure tremendous stress from temperature extremes and damaging neutrons. The magnetic fields will exert forces equivalent to those seen only in very high pressure chemical reactors and specialized laboratory equipment. All in all, the engineering will be extremely complex.

A working fusion reactor would also have to be very large. This conclusion is based on fundamental principles of plasma physics and fusion technology. To begin with, because of the properties of magnetic fields, a fusion reactor must be tubular. There is still dispute as to whether this tube should be bent into a toroidal (doughnut) shape, as in the device known as the "tokamak," or kept as a long, straight tube with end plugs, as in the device known as the "tandem mirror." However, the main conclusions as to the size and complexity of a D-T reactor are independent of this choice.

A fusion reactor might well produce only one-tenth as much power as a fission reactor of the same size.

The first wall of the reactor encloses the plasma. The best theories available suggest that the radius of the plasma must be at least two to three meters if the fusion reaction is to be self-sustaining. Even if a breakthrough in physics were to allow a smaller plasma, separate engineering requirements would prevent the radius of the first wall from being appreciably less than three meters. These requirements arise from the need to avoid excessive differences in power density.

For the neutrons to be slowed enough in the lithium to effectively breed tritium, the blanket surrounding the first wall must be between half a meter and one meter thick. The radiation shield outside the blanket must also be between half a meter and one meter thick to protect the supercooled magnets. Finally, the superconducting magnets and their structure will add another meter each to the radius. That gives a total radius of at least five meters for the plasma and the tube surrounding it.

In a tokamak reactor, this tube—over 30 feet across—would be bent into a doughnut-like shape at least 75 feet in outer diameter. As a power plant, this is somewhat larger than today's fission reactors and substantially more complex. If the energy density of the fusion plant turned out to be lower than that of a contemporary fission plant, as seems likely, then all this size and complexity would produce less power—hardly an economic proposition. But even if the power density were comparable, the D-T fusion reactor would, like today's fission plants, be a large and costly power source, producing thousands of megawatts of electricity. Detailed studies, some costing millions of dollars, aimed at deducing the smallest plausible size for a D-T fusion reactor all come to this same discouraging conclusion.

Such a large reactor would not meet the needs of utilities. Plagued by financially crippling cost overruns on fission reactors, managers are loathe to invest several billion dollars in any single plant, fission or fusion. Smaller plants, such as coal plants with scrubbers, are much easier to finance, not only because the investment is far lower, but also because the final cost is predictable. And if a small plant breaks down, the effects on regional electricity production are much less serious. Thus, utility managers find large plants undesirable.

Suppose fusion reactors could be built despite the inherent difficulties of size and complexity. Another critical engineering problem would still have to be faced. That is the matter of heat transfer—the way in which heat is removed from the reactor structure by the circulating coolant. The history of much large-scale power engineering has been dominated by the effort to achieve ever higher temperatures and heat-transfer rates. High temperatures imply high efficiency, and high heat-transfer rates imply high power density. Because these goals are so desirable, heat-transfer systems have been pushed close to their limits. Above these limits, materials either melt or fail from excessive stress caused by heat. Additional gains are coming only slowly.

Consider heat transfer in fission and fusion reactors. In today's typical light-water reactor (LWR), heat is generated by fission in fuel pins containing uranium. The heat is then transferred to the coolant at the surfaces of a relatively large number of small-diameter pins. This arrangement provides a larger surface area to transfer heat than, say, a single large fuel cylinder. Indeed, by decreasing the diameter of the pins even further (but increasing their number to keep the amount of uranium unchanged), the total surface area available to transfer heat would be further increased. Thus, the actual heat-transfer rate through any given square inch of surface on a fuel rod is not critical. Sufficient heat can always be removed merely by increasing the total area.

This strategy does not work in a fusion reactor. The heat-transfer surface is limited to the inside of the wall surrounding the plasma, and the relatively small surface area of this wall cannot be increased without further increasing the size of the reactor. In fact, bigger reactors need *larger* heat-transfer rates. Thus, the actual heat-transfer rate per square inch must be extremely large and cannot simply be reduced by a design change.

Suppose a fission reactor and a fusion reactor were built with equivalent heat-transfer rates. Knowing this, one can calculate two other critical engineering factors: the flux of neutrons at the heat-transfer surface, and the overall power density of the reactor. The neutron flux should, of course, be as low as possible, because it damages the reactor structure and makes it radioactive. And the power density should, as mentioned, be as high as possible, so that a reasonable amount of power will be produced in a reactor of a given size.

On these counts, a comparison between current LWR fission reactors and the somewhat optimistic fusion designs produced by the DOE studies yields a

The drawbacks of the existing fusion program will weaken the prospects for other fusion programs, no matter how wisely redirected.

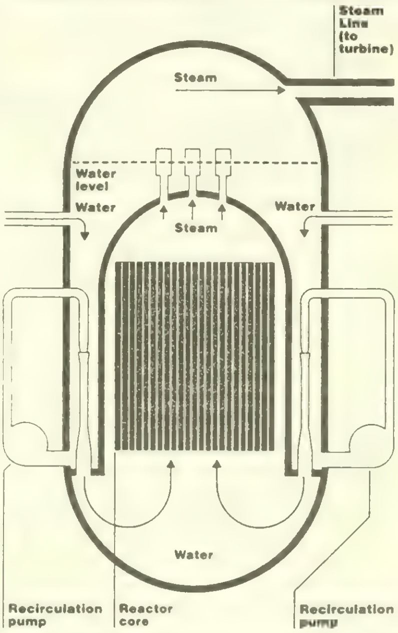
devastating critique of fusion. For equal heat-transfer rates, the critical inner wall of the fusion reactor is subject to ten times greater neutron flux than the fuel in a fission reactor. Worse, the neutrons striking the first wall of the fusion reactor are far more energetic—and thus more damaging—than those encountered by components of fission reactors. Even in fission reactors, the lifetimes of both the replaceable fuel rods and the reactor structure itself are limited because of neutron damage. And the fuel rods in a fission reactor are far easier to replace than the first wall of the fusion reactor, a major structural component.

But even though radiation damage rates and heat-transfer requirements are much more severe in a fusion reactor, the power density is only *one-tenth* as large. This is a strong indication that fusion would be substantially more expensive than fission because, to put it simply, greater effort would be required to produce less power.

Fusion's Benefits

Given all of fusion's liabilities, why are we working so hard on it? The universal availability of fuel has provided a strong motive to develop fusion, and it does promise some other substantial advantages over fission. To begin with, fusion generates much less radioactivity than fission, and there is no long-term storage problem for radioactive wastes. A fusion reactor would create a lot of tritium, which is radioactive and hard to contain. However, tritium's biological effects are relatively benign—it does not tend either to concentrate or to linger in living organisms—and it emits relatively weak radiation. After a short period of operation, the radioactivity from neutrons bombarding the structure of a fusion reactor itself would greatly exceed the feeble radioactivity of the tritium.

But even the radioactivity of the structure will be composed primarily of nonvolatile isotopes. By contrast, a substantial amount of the radioactivity in fission reactors is in the form of volatile gases that can escape if the containment structure is breeched. To further minimize the radioactivity associated with fusion, reactor designers can choose structural materials that do not become strongly radioactive when bombarded by neutrons. A fusion reactor of stainless steel would have 300 times less radioactivity than a fission reactor of the same power output. A fusion



In a fission reactor, heat from the reaction is released through the surfaces of thousands of fuel rods. Additional surface area to transfer heat can be created by providing more fuel rods but making them thinner.

However, in fusion plants, the 150,000,000° C plasma is encircled by a "first wall," the surface of which cannot be increased in any practical way. (If the encircling wall is made bigger, then the larger plasma creates even more heat.) Thus, as much energy as possible must be transferred through each square inch of the first wall. Unfortunately, improvements in heat-transfer rates are coming only slowly.

Neutrons induce radioactivity and damage reactors. Neutron-free fusion might provide inexhaustible, benign power.

reactor based on a vanadium structure would be 10 times better yet. In other words, it seems possible to build a fusion reactor with 3,000 times less radioactivity than a fission reactor producing the same amount of power.

The radiological difference between fission and fusion is even more striking in the production of long-lived wastes. There is nothing in the fusion reactor comparable to the fission fragments or the plutonium in fission reactors. Plutonium is extremely hazardous and its radioactivity is very long-lived, with a half-life of 24,100 years. After a 100-year storage period, the radioactive waste produced by a stainless-steel fusion reactor would be 1 million times less hazardous than that produced by an equivalent fission reactor. And there would be no need to store the waste of a fusion reactor with a vanadium structure even that long. A well-designed fusion reactor could completely eliminate the problem of storing long-term waste.

The fact that a fusion reactor does not require long-term waste storage seems a clear advantage. But it is less significant than would first appear, for we have tended to exaggerate the waste-storage problems of fission reactors, primarily because of ill-considered decisions early in their development. Early schemes for disposal of fission wastes had to be inexpensive to allow the reactors to compete with conventional power plants fueled by inexpensive oil. Early schemes for disposing of the wastes—dumping them on the ocean bottom or injecting them into underground strata—were certainly cheap. However, these schemes were so clearly inadequate that the fission community did its reputation lasting damage by advocating them.

Although the public is still concerned about the disposal of radioactive waste, the economic situation is now completely changed. Fission products can be safely stored, as is routinely done in Europe now. To be sure, such processes are not inexpensive. For example, one technique consists of sealing intact fuel elements in welded metallic canisters and storing them in mined granite cavities. If better techniques for storage should become available, the wastes can be retrieved. The costs of such relatively expensive disposal still play only a small role—less than 10 percent—in the total price of power. Public perception changes slowly, but the time scale under consideration is long. Waste disposal will eventually be considered a difficult but not insurmountable problem.

The matter of safety is difficult to weigh so concretely. Current analyses show that the probability of a minor mishap is relatively high in both fission and fusion plants, because both contain many complex systems. But the probability of small accidents is expected to be higher in fusion reactors. There are two reasons for this. First, fusion reactors will be much more complex devices than fission reactors. In addition to heat-transfer and control systems, they will utilize magnetic fields, high power heating systems, complex vacuum systems, and other mechanisms that have no counterpart in fission reactors. Furthermore, they will be subject to higher stresses than fission machines because of the greater neutron damage and higher temperature gradients. Minor failures seem certain to occur more frequently.

Comparing the probability of more serious accidents is harder, partly because that issue is the subject of such heated debate concerning fission reactors. But the probability of major accidents affecting public safety will certainly be substantially lower for fusion reactors. Indeed, the hypothetical worst-case accident of a fission reactor—catastrophic meltdown with release of fission products—has no equivalent in fusion. The fusion reactor simply does not contain enough radioactive material.

Thus, fusion reactors will have a higher probability of small accidents but a much lower probability of major accidents. This at first appears to be a strong argument for fusion, but consider Three-Mile Island. This accident, thought by some to have sounded a death knell for the fission industry, may have had equally damaging consequences for fusion. Although no one was physically injured in the TMI accident, the utility owning the reactor was mortally wounded financially. The multi-billion-dollar plant was put out of commission because it was too radioactive to repair. From a manager's standpoint, all systems that are too radioactive for hands-on maintenance are equivalent: if something major breaks, it is unrepairable. Although there is much less radioactivity in a D-T fusion reactor than in a fission reactor, it is still so high that contact maintenance would be impossible. And a D-T fusion reactor would be far more likely than a fission reactor to require repairs.

The analysis of safety factors comes down to this: While the public is primarily concerned about major catastrophes, power-plant operators are also fearful of less threatening accidents that could cause serious financial problems. In respect to these, fusion is at a

Neutron-Free Fusion

ALMOST all of the lighter elements are capable of entering into fusion reactions in which the nuclei of atoms are combined and energy is released. The prime candidates for power-producing reactions are based on two isotopes of hydrogen: protons (p), which are the standard hydrogen nuclei; and "heavy hydrogen," or deuterium (D), which has a neutron attached to the proton. The nuclei of the hydrogen isotopes have the lowest possible electric charge—one positive charge. Thus, they require lower energies to be brought together for fusion reactions than other nuclei with larger positive charges.

The original proposal for fusion was to produce power through the self-fusion of deuterium—the D-D reaction. This reaction produces, with equal probability, either the light helium isotope with two protons and a neutron (He^3) or the heaviest hydrogen isotope, tritium (T), with one proton and two neutrons. Both reactions release energy, generally measured in millions of electron volts (MeV).

$$D + D \rightarrow He^3 + n + 3.2\ MeV$$

$$D + D \rightarrow T + p + 4.0\ MeV$$

These reaction products can themselves react with deuterium and will either be "burned" in place or recycled.

$$D + T \rightarrow He^4 + n + 17.6\ MeV$$

$$D + He^3 \rightarrow He^4 + p + 18.3\ MeV$$

Because the fuel for the last two reactions is generated in the first two, only deuterium need be supplied externally. The final reaction products—ordinary helium and hydrogen—are benign, but the energetic neutrons can damage and induce radioactivity in the structure of the reactor.

Fusion based on *any* fuel cycle containing deuterium produces undesirable neutrons. The reason is this: Most of the deuterium can be made to "burn" in a desired reaction—for example, the benign D-He^3 fusion above, to produce ordinary helium, a proton, and energy. But some of the deuterium in the mixture will also collide with itself, producing neutrons and radioactive tritium; further collisions with the tritium will produce more neutrons.

Fuel cycles based on protons tend to produce far lower amounts of neutrons. The lithium-6 reaction

$$p + Li^6 \rightarrow He^3 + He^4 + 4.02\ MeV$$

is often considered because of the low charge of both constituents. But it is not completely neutron-free. A product (He^3) can react with Li^6 to produce neutrons via a low-probability, but nonetheless troublesome, side reaction.

From an engineering point of view, the boron-11 reaction

$$p + B^{11} \rightarrow 3He^4 + 8.68\ MeV$$

is nearly ideal. Neither the fuel *nor* the end products are radioactive. Furthermore, no neutrons capable of inducing radioactivity are produced.

Because all the products of the boron-11 reaction are charged, they could theoretically be harnessed to generate electricity directly, without the inherent waste of generating steam to run a turbine. However, the high electric charge of boron (it has 5 protons) makes the task of designing an energy-producing system very difficult.—*L.L.*

disadvantage. If this factor is added to the reactor's high initial cost, large size, and poor power density, D-T fusion becomes an unacceptable financial risk. The public perception of fusion as ultimately safer than fission cannot nullify this.

Furthermore, in a broader sense the safety of a D-T fusion reactor would depend on its being used responsibly. One of the best ways to produce material for atomic weapons would be to put common, natural uranium or thorium in the blanket of a D-T reactor, where the fusion neutrons would soon transform it to weapons-grade material. And tritium, an unavoidable product of the reactor, is used in some hydrogen bombs. In the early years, research on D-T fusion was classified precisely because it would provide a ready source of material for weapons. Such a reactor would only abet the proliferation of nuclear weapons and could hardly be considered a wise power source to export to unstable governments.

A major driving force behind fusion has been the promise of abundant fuel. Indeed, the fusion program was originally justified not on safety grounds—

The scientific goal of the fusion program turns out to be an engineering nightmare.

fission's safety was not widely doubted then—but because of the expected rapid depletion of uranium reserves. But this is no longer a major concern. One reason is the declining demand for additional fission power and hence for the uranium to fuel it. The earth's reserves of uranium are now known to be large enough to supply fission reactors for at least 50 to 70 years without fuel reprocessing.

There has also been a breakthrough in the technology for removing uranium from seawater. A Japanese consortium is starting up a pilot plant that uses an efficient filter to trap and concentrate the extremely dilute uranium in seawater. This technology will make available virtually unlimited supplies of uranium at a cost at most ten times the current (depressed) price for conventionally mined uranium. The cost of nuclear fuel is so small a fraction of the total cost of generating electricity that the new technology would increase electricity prices only negligibly. The same oceans that could supply fusion fuels can also supply fission fuel; the abundance of deuterium for fusion ceases to be a compelling argument.

Dim Prospects for D-T Fusion

In retrospect, it is not totally surprising that fusion should fare so poorly in comparison with fission. The problem is simply that in fusion, 80 percent of the energy is released in neutrons with an energy of 14 million electron volts (MeV) that travel about 50 centimeters. In fission, less than 3 percent of the energy is released in neutrons, and these have an energy of only 1 to 2 MeV. Most of the fission products are highly charged nuclei that travel less than .001 centimeter before coming to rest.

Thus, while the major radioactivity from fission is contained within the fuel pins, the major radioactivity from fusion would damage the reactor structure and create problems of complexity, unreliability, and size. While fission's numerous wastes pose problems of disposal and reactor safety, fusion's neutrons could easily be used to manufacture material for atomic weapons. It is hard to see why a utility in need of additional generating capacity would purchase a D-T fusion reactor instead of a contemporary LWR fission reactor. And as far as most utilities are concerned, even the LWR no longer seems a good choice.

The early history of the fission program was similar to current experience in the fusion program—except that success in fission came too easily. As soon as we found a concept that worked reasonably well, powerful forces drove that machine, the LWR, to prominence. We did not take the time to test, modify, and finally choose the "best" nuclear reactor among many competitors.

Now we know that safer, smaller, and probably cheaper fission reactors can be built. In fact, reactors could be small enough to be assembled in a factory and shipped via truck, reactors so safe that no operator error or even loss-of-coolant accident could cause release of radiation. The dreaded meltdown would also be impossible in these small, "modular" reactors. Such a reactor has been operating for 15 years in Germany. To be sure, this kind of reactor would probably not be the best choice in a world in which uranium was scarce and reprocessing and fuel breeding were necessary. But we do not live in such a world. Unfortunately, the resounding crash of the LWR has prejudiced the possibility of a new beginning for fission reactors.

The only real hope for fusion is to take the long view ignored in the fission program. Neutron-free fusion is a quintessential example of a high-risk, high-gain area of physics that *might* also provide a good answer to an engineering problem. We have no guarantee that an answer exists. But we know that if it does, it can meet the original goal of the fusion program—universally available, inexhaustible, environmentally benign power. Perhaps we should not be greatly troubled that our first attempt to develop such a marvelous thing will not be the success we had hoped. We can go on to seek a better alternative.

LAWRENCE M. LIDSKY, *professor of nuclear engineering at M.I.T., is an associate director of the Plasma Fusion Center and editor of the* Journal of Fusion Energy. *He has worked on plasma physics and fusion-reactor technology for 20 years.*

Lawrence Livermore National Laboratory

March 6, 1996

The Honorable Dana Rohrbacher
Subcommittee on Energy and Environment
Committee on Science
U.S. House of Representatives
374 Rayburn House Office Building
Washington, DC 20515

Dear Representative Rohrbacher:

I thank you and the Subcommittee on Energy and Environment of the House Science Committee for the opportunity to submit this written statement for the record of your hearings on the Department of Energy's restructured Fusion Energy Science Program. As the leader of inertial fusion research at the Lawrence Livermore National Laboratory, I strongly support fusion energy research in general including the continuation of the important work on advancing the Tokamak concept. I also support the Department's Fusion Energy Advisory Committee (FEAC) report "A Restructured Fusion Energy Sciences Program", including its recommendations for increased emphasis on fusion science, on international collaboration, and on alternatives to the Tokamak. In the context of this last recommendation, I want to urge you to include a viable Inertial Fusion Energy (IFE) component within the newly restructured program. The IFE component should focus on energy-related aspects of inertial fusion that are not within the scope of DOE/Defense Programs (DP) and be large enough and structured to take advantage of the leverage offered by DP's inertial fusion program.

Background:

The initiation of the National Ignition Facility (NIF) Project this year by DP's inertial fusion program along with continuing advances in the Inertial Confinement Fusion (ICF) Program brings with it the prospect that, for the first time, a small capsule of fusion fuel will be compressed and heated by the world's largest laser to achieve fusion ignition and energy gain in the laboratory within the next decade. This event could have dramatic effects in several areas.

The primary mission of the NIF, and DP's inertial fusion program which provides the scientific and technology basis for the NIF, is to study fusion ignition and extremely high temperatures and densities in matter in support of the Stockpile Stewardship and Management (SSM) Program. DP's stewardship program must assure the safety, reliability, and performance of the remaining nuclear weapons in the stockpile without nuclear weapon testing. The temperatures and densities that can be achieved in NIF experiments are very close to those that exist inside operating nuclear weapons and inside the sun and other stars. The NIF experiments will study conditions of weapons physics and weapon effects that cannot be achieved in any other way without nuclear weapon tests. The NIF experiments, along with experiments on other stewardship facilities, will help develop and verify advanced computer models and be used to increase understanding of complex weapon processes, and evaluate and fix problems that will arise in specific weapons as these weapons age. SSM is a responsive and cost-effective Program required to maintain the nuclear deterrent while advancing the United States Arms Control and Nuclear Non-Proliferation agenda.

NIF high temperature and density experiments will significantly expand opportunities in astrophysics, plasma physics, and other basic sciences. These disciplines will be able to examine processes that occur in the center of stars and other topics of interest in high energy density physics in the laboratory for the first time.

The same world class science that supports stewardship will also allow us to design and evaluate the fusion targets that will be needed for future IFE power plants, regardless of what driver is used for them. Studying ignition targets would establish the fundamental feasibility of inertial fusion as a power source by telling us what size of plant is likely to be practical and economic. The NIF would also allow study of some of the technology developments necessary to make IFE power plants a reality. Possible designs for IFE power plants have been investigated for some time. A book published in March, 1995, by the International Atomic Energy Agency entitled "Energy from Inertial Fusion", describes the great progress made in inertial fusion science and technology in inertial fusion research laboratories around the world, and how that science and technology can lead to a variety of inertial fusion power plants with attractive economic and environmental characteristics. The book describes scientific and technical features, possible development pathways, and power plant designs for IFE. This book shows the high leverage fusion energy development will accrue by leveraging on DP's inertial fusion program.

The National Academy of Sciences, the JASONs and other review groups have recommended that the NIF study inertial fusion science at minimum cost using glass lasers capable of firing every few hours. Other technologies will be needed for future IFE power plants. The principal one is an efficient driver to supply intense ion or laser beams (drivers) that can be fired several times a second. Present research should focus on resolving the remaining uncertainties about the feasibility of drivers for IFE by the time ignition is achieved on NIF. Only by doing so can the best decisions be made about the future of inertial fusion as an energy source.

Needed research for IFE:

The FEAC recognized that a relatively small effort on fusion-energy specific technologies in the new program would compliment and leverage the inertial fusion research in DP that is dual-benefit. A synergistic partnership between the two programs would provide a unique opportunity to develop fusion energy in a technically correct and cost-effective manner. However, as the FEAC report also noted, the present budget of 8 million dollars allocated to IFE is insufficient to support experiments that could prove out all the critical issues of heavy-ion accelerator drivers needed for IFE, nor enough to cover research on other promising driver candidates, and on materials and chamber technologies needed for IFE, which are not fully covered as part of DP's scope.

I support the FEAC recommendation that the IFE Program be reviewed by appropriate experts in light of the newly restructured fusion energy program. A program with deliverables, decision points, schedule and budget, recognizing that IFE is the principle alternate to Tokamaks, should result from this review. Previous FEAC reviews have recommended budget levels in the range of twenty million dollars to optimally exploit DOE-Defense Program's ICF activities in operating and NIF construction (which total approximately four hundred million dollars per year through 2002). I believe that a new programmatic review of IFE will justify support for IFE at this level, with comparable support for magnetic alternates.

The ICF Program is prepared to present its case to such a review on the appropriate respective research activities in Defense Programs and fusion energy sciences that would most efficiently advance inertial fusion energy and the U.S. overall fusion energy effort. I know that Defense Programs and the inertial fusion community strongly support the pursuit of IFE in the new fusion energy science program as the leading alternative to the Tokamak, and will be a willing and supportive partner to coordinate the needed research for the success of IFE.

Thank you for the opportunity to express my views.

E. Michael Campbell

E. Michael Campbell
Associate Director, Laser Programs
Lawrence Livermore National Laboratory

GENERAL ATOMICS

March 18, 1996

The Honorable Harold L. Volkmer
Member of Congress
2409 Rayburn House Office Building
Washington, D.C. 20515

Dear Congressman Volkmer:

I understand that during the March 7, 1996 hearing on fusion conducted by the Subcommittee on Energy and Environment, the suggestion was made by two witnesses that General Atomics, as a private company, should be required to privately finance its fusion research. During the hearing you suggested that General Atomics might want to comment on this idea. This letter is an attempt to do so.

Fusion energy clearly meets the criteria of a research area for which federal government is both appropriate and necessary. The potential benefit to society is very high. The global need, measured on a fifty year time scale, is incontestable. The cost of this research and development is very high and the risk of failure on any given approach is real. The payoff on investment is beyond the time horizon of any individual company, including General Atomics. For all of these reasons, fusion research is supported by virtually every developed nation of the world.

Given the appropriateness of federal funding for fusion, the question then is who is to do it. Fusion is both scientific and technically challenging -- it has been called the most difficult such endeavor ever undertaken. It requires large facilities and large research teams. Success requires that its research be goal oriented and it also requires the efforts of more than one generation of researchers. For reasons like these, the Department of Energy national laboratories have played a prominent role in fusion research; universities and small industrial laboratories simply are mismatched to the task, except as support.

Last year, the "Galvin Commission", chaired by Motorola Chairman Robert Galvin, studied the Department of Energy multiprogram laboratory system and made several recommendations for improvements in its effectiveness. Among these was a recommendation to convert the laboratories to private entities having a contractual relationship with the DOE which would retain programmatic and fiscal control but be out of the direct management chain. The benefits seen were increased accountability, cost-effectiveness, flexibility, etc.

General Atomics has played a leading role in fusion research for more than 35 years. Currently, GA hosts the DIII-D tokamak as a national and international facility, which is used by collaborative teams from almost fifty U.S. and foreign laboratories and universities. The DIII-D is a modest size tokamak which, because of its advanced design, competes successfully with the largest in the world. In fact, DIII-D is looked to as pointing the way for tokamak research directions for the future.

3550 GENERAL ATOMICS COURT, SAN DIEGO, CA 92121-1194 PO BOX 85608, SAN DIEGO, CA 92186-9784 (619) 455-3000

The Honorable Harold L. Volkmer
page 2

General Atomics' fusion work has been supported by the Department of Energy and its predecessors in a model very much like that recommended the Galvin Commission. GA has a reputation for on-time-and-cost project management and its manpower cost rates are below national lab equivalents. All of our fusion research results supported by the DOE are public-domain information: GA receives no competitive advantage. I have argued in the past that GA is model of what "Galvin had in mind". Rather than be labeled as an example of "corporate welfare", I argue that we provide a very viable model for large-scale federal R&D for the future.

Thank you for suggesting that General Atomics address the suggestion of private financing for fusion research. I hope that this letter will prove helpful to you and the Subcommittee.

Sincerely,

David E. Baldwin

Dr. David Baldwin
Senior Vice President, General Atomics

cc: The Honorable Dana Rohrabacher,
Chairman, Energy and Environment Subcommittee

March 21, 1996

The Honorable Dana Rohrabacher
Chairman, Subcommittee on Energy and Environment
B-374 Rayburn House Office Building
Washington, D.C. 20515

Dear Chairman Rohrabacher:

We are writing to address concerns you raised in your opening remarks at the March 7 hearing of the Energy and Environment Subcommittee on the restructured Fusion Energy Sciences program recommended by the Fusion Energy Advisory Committee (FEAC). Specifically, these concerns related to the relative value and importance of tokamak experiments in advancing the scientific knowledge base needed for the ultimate development of fusion energy when compared to other alternative fusion concepts.

It is our belief that the tokamak should be viewed as a necessary and valuable step towards the ultimate goal of economic and competitive fusion energy. This view is based on the tokamak's role, not yet completed, as a tool for developing fusion science; its own potential for fusion power; its ability to provide a testbed for fusion technologies; and its contributions to the science of many alternative, non-tokamak, approaches through the transferability of its science base.

The fact that success would require developing a thorough understanding of fusion science was recognized early in fusion research. The program of the last decade or more has been devoted to this investigation and it has borne astonishing success, arising from a convergence of new and powerful diagnostics, theoretical understanding, and computational modeling. Because of the tokamak's capacity for holding heat and its effectiveness in achieving the required magnetic field configuration, it has proved by far the best research tool for achieving fusion conditions in the laboratory, a prerequisite for studying and developing the science of fusion plasmas. Today there is a world-wide consensus that the tokamak is the correct vehicle for achieving a self-heated burning plasma, for developing the technologies essential for a power plant, and for testing relevant components.

The criteria for an economic power plant will be more demanding than those for an experimental facility. Although there is good basis for confidence that a plant modeled on today's "conventional tokamak" would work technically, the cost of electricity produced might be high compared to fission or fossil fuels, owing to its low power density, possible limitation to pulsed operation, engineering complexity, etc. The "advanced tokamak" line of research, which is the thrust of current U.S. research in tokamaks, seeks to improve the tokamak by exploiting its confinement physics. Just as important, the advanced tokamak provides a cost-effective means to access the fusion plasma physics.

The plasma science developed through tokamak research applies directly to a variety of other toroidal "cousins" of the tokamak, e.g., the stellarator, spheromak, reversed field pinch, etc., which might offer advantages over the tokamak as a power plant. (These toroidal cousins form an important subset of the alternatives to the tokamak for fusion power. Others are not so dependent on tokamak science, e.g., inertial fusion energy builds on the DOE Defense Program's Inertial Confinement Fusion research.) These toroidal alternates lagged in performance in comparison to tokamaks during the early days of fusion research. Therefore, as a consequence of this lower performance, they were not supported at a level sufficient to develop an understanding of their confinement properties. With our improving understanding of high-temperature plasma science and associated technologies, these alternatives should be reconsidered. The science base that continues to be developed in the tokamak program provides the means for this reassessment.

Offsetting the perceived attractiveness of the alternatives is a higher level of technical risk. Any new experiments on toroidal alternatives would first address specific scientific issues which did not require fully fusion-grade plasmas, and their facilities would be correspondingly less costly than today's tokamaks which are in an advanced stage of development. Should it appear worthwhile to pursue research in a particular alternative to higher performance levels, larger facilities would be needed in the future. However, because this alternative would benefit from the tokamak research and because it would, by assumption, have advantages over the tokamak, its overall development costs should be lower.

Research using other magnetic configurations benefits the fusion program in two ways. First, while we believe that the tokamak's attractiveness as a power source can be considerably improved, it is our hope that the advantages offered by one or more of the alternatives can lead to a superior product. Second, research in these other configurations enriches and advances our understanding of tokamaks, as well as fusion plasmas in general. Therefore, the best chance for developing a truly attractive fusion power plant is provided by a balanced program, combining strong continued tokamak research with a healthy program in alternatives as part of a well directed and strongly supported domestic program in plasma science, as recently recommended by the Fusion Energy Advisory Committee.

We respectfully request that this letter be placed in the March 7th hearing record and hope it will prove helpful to you as the Science Committee considers the fusion program in the weeks to come. Thank you for your consideration.

David E. Baldwin
Senior Vice President, Fusion Group
General Atomics

Charles C. Baker, Professor
University of California, San Diego

Bruno Coppi, Professor
Massachusetts Institute of Technology

Ronald C. Davidson, Director
Princeton Plasma Physics Laboratory

Steven O. Dean, President
Fusion Power Associates

Jim Drake, Professor
University of Maryland

Richard D. Hazeltine
Director, Institute of Fusion Studies
University Texas @ Austin

Arnold H. Kritz, Professor
Lehigh University

Michael E. Mauel, Professor
Columbia University

Francis Perkins, Jr.
Senior Staff Scientist
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Miklos Porkalab, Professor
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Michael J. Saltmarsh, Director
Fusion Energy Division
Oak Ridge National Laboratory

Andrew M. Sessler
Distinguished Staff Scientist
Lawrence Berkeley Laboratory

Richard E. Siemon
Fusion Energy Program Manager
Los Alamos National Laboratory

Thomas H. Stix, Professor
Princeton University

Keith I. Thomassen
Deputy Associate Director, MFE
Lawrence Livermore National Laboratory

cc: The Honorable Tim Roemer, Ranking Minority Member
Subcommittee on Energy and Environment

SECC

Safe Energy Communication Council
1717 Massachusetts Ave. NW
Suite 805
Washington, D.C. 20036
(202) 483-8491
FAX (202) 234-9194

March 28, 1996

Dana Rohrabacher
Chairman
Subcommittee on
Energy and Environment
Committee on Science
B-374 Rayburn House Office Building
Washington, DC 20515

Dear Chairman Rohrabacher:

On behalf of the Safe Energy Communication Council (SECC), I appreciated the opportunity to participate in the March 7, 1996 Subcommittee hearing on the Department of Energy's Restructured Fusion Energy Sciences Program. I have received a copy of the transcript and some additional questions from Members of the Subcommittee.

With respect to the transcript, I would like to submit a clarification for the record. The response to the question from Representative Johnson on page 131 and 132 of the transcript concerning private sources of funding for tokamak reactors was inconsistent with my prepared testimony. SECC is not advocating public/private partnerships to pay for the fusion facilities at San Diego and the Massachusetts Institute of Technology.

The question and response is as follows:

[[Ms. Johnson. --I would like to ask Mr. Adams and Mr. Schatz, could you identify some of the private sources that some of the programs could access to fund some of the projects privately?

Mr. Adams. Well, with the General Atomics run facility, obviously we would look towards them. I mean, we need to have some sort of industry cost share here. I mean, the point is that if these facilities are performing valuable research, that is one thing. The problem is that I just don't think the taxpayers can continually support these for year after year. So for at least the San Diego facility, we would look at General Atomics itself to pick up some

Safe Energy Communication Council

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of the fund, because I mean this is a commercial entity that is operating a facility. And, it seems to make sense for them to be picking up the cost.

Ms. Johnson. Well, let me--you mean, a pubic/private partnership or the ceasing of federal funds altogether?

Mr. Adams. Well, we think both should be looked at. Perhaps it could be phased out over a period of years or cost share could occur over several years. We haven't really made a definitive thing one way or another. We would at least like to at least see it go in that direction. And, ideally, at some point, we would like to see it go in that direction.]]

The response to Representative Johnson's question was inaccurate when compared with the prepared testimony. Therefore, we request that a footnote be inserted at the end of this passage in the transcript and the following clarification should be inserted in the record in the appropriate location:

Regarding further support for tokamak reactor facilities, the second recommendation in SECC's prepared testimony states the following:

> "The Tokamak Fusion Test Reactor at Princeton University should cease operations in FY 1996 and additional funding for FY 1997 should be restricted to termination costs. The operation of DIII-D beyond the current fiscal year should be financed by private industry. Similarly, an alternative source of funds should also be identified for the Alcator facility without relying on further support."

Thank you for the opportunity to review and clarify the record. The responses to the additional questions raised will be submitted to the Subcommittee next week.

Sincerely,

James Adams
Senior Analyst

APPENDIX II

UNIVERSITY OF CALIFORNIA, SAN DIEGO

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March 29, 1996

Representative Dana Rohrabacher, Chairman
Energy and Environment Subcommittee
Committee on Science
U. S. House of Representatives
Room B374, Rayburn House Office Building
Washington, DC 20515-6301

Dear Congressman Rohrabacher;

I am writing in response to your letter of March 15, 1996, requesting answers to three additional questions, posed by Members of the Subcommittee, following my testimony on March 7, 1996.

My testimony centered on the recommendations of the recent National Research Council study of plasma science. [1] The following are the views of my panel co-chair, Dr. John Ahearne, and myself, since the Report did not specifically address these questions. However, we believe that the following responses are consistent with the findings and recommendations of the NRC Report.

You asked three questions, and I address them in order.

1. *What do you believe are the reasons why plasma science is not adequately recognized as a discipline?*

The present funding structure for physical sciences in the United States was established following World War II. This funding was organized along the lines of then-established scientific disciplines. In this context, plasma science was not identifiable at the time of the post World War II science initiatives. It really began in earnest in the late 1950's and in the 1960's with the growth of the fusion and space programs, although some aspects were in place much earlier, such as work on electron-beam devices done in the 1940's. As researchers returned from their war work, they continued plasma-related studies, but within many different university departments and professional societies, including physics, electrical engineering, and astronomy. Consequently, plasma science was not recognized as a discipline, but seen through its applications.

In the past two decades, many applications of plasma science have become recognized as important to our society. They include the plasma processing of semiconductor chips, the use of plasmas for environmental clean up, the quest for useful fusion energy, and understanding space plasmas for global communications. Thus, many important applications are being vigorously pursued, but until now, the United States has not taken the important step of establishing an organized funding structure for the basic plasma science that feeds all applications. The present restructuring of the fusion energy program provides a unique opportunity to establish this funding base. The NRC report [1] provides a blueprint for the breadth and scope of such a program and a vision of what this program, once established, could provide to the Nation.

2. *What do you see as current major obstacles [in the path to the economic generation of electricity using fusion energy]?*

The projected size of a commercial fusion power plant has grown considerably over the years, so that even a pilot project such as ITER strains the currently envisioned budget for such efforts. Moreover, the favored design for the first device that is expected to create large quantities of fusion energy is a tokamak. It is highly questionable whether this approach will lead to a commercial power plant that will generate electricity economically.

There are many scientific and engineering questions that must be addressed by research and exploratory development in order to be confident that fusion can provide economically competitive electricity. Given the size of the next generation of devices, addressing the many technical questions must be done carefully, in order that these activities are affordable in the context of the budgets that one can sensibly expect.

3. *How can we best assure that the such earmarking [of money for basic plasma science] does indeed address "the most important basic plasma science questions"?*

This issue is crucial in establishing an effective and efficient support structure for basic plasma science. We believe the critical factor in ensuring that a program in the Department of Energy addresses the most important scientific questions is that plasma science needs an identifiable home in the Department. There needs to be a knowledgeable person [or organizational unit] who has the specific responsibility for this basic plasma research program.

The NRC report recommends a broadly based research program in plasma science in the Department. Therefore, this person [or organizational unit] should have responsibility for seeing that all aspects of basic plasma science are addressed; ranging, for example, from the physics of high-temperature, collisionless plasmas to low-temperature, chemically reactive plasmas. It is the synergy of ideas and scientific talent from the many areas of plasma science that will give the program the strength and breadth to identify and address the fundamental questions that can be of most benefit to the many applications of plasmas. If one person or entity does not have responsibility for the entire program, the program can too easily be diverted to support just one or a few specific applications. While this latter course will be beneficial, it will not be the most effective use of the Nation's talent and other resources for the good of our society. The individual [or unit] responsible for this program would be aided substantially if Congress were to "fence" the money to be specifically allocated for a broadly based program in basic plasma science.

This effort in basic plasma science must be long-term in order to be effective. It takes several years to create a research effort -- to plan and build experiments and to assemble and train the personnel to conduct the research. Plasma science will not be able to attract high quality researchers to the field, if there is not a commitment of the Department of Energy to a stable, long-term program in basic research.

It is also important to note that one can easily expend all of the available resources for this program and have little to show for it, if the research is done on large projects, where near-term and other priorities will almost certainly dominate. The NRC report [1] recommended that the program emphasize basic, university-scale, experimental research. The Panel concluded that this was the most cost-effective way to address the majority of basic plasma science issues, and it found that this was the key ingredient most lacking in the United States' present support of plasma science.

If I can be of further help in your work in the restructuring of the fusion energy program or with other questions regarding plasma science, please feel free to contact me at the addresses and numbers given above.

Sincerely yours,

Clifford M. Surko
Professor of Physics
Chair, Panel on Opportunities in Plasma Science and Technology,
National Research Council

---

1. National Research Council Report, *Plasma Science: From Fundamental Research to Technological Applications,* National Academy Press, Washington D.C., 1995.

Copy to:

J. Ahearne
J. E. Jensen
D. C. Shapero
R. Sudan

FUSION RESEARCH CENTER
THE UNIVERSITY OF TEXAS AT AUSTIN
Robert Lee Moore Hall • Austin, Texas 78712-1068 • (512) 471-4698 • FAX (512) 471-8865

April 2, 1996

The Honorable Dana Rohrabacher, Chairman
Energy and Environment Subcommittee
Committee on Science
U.S. House of Representatives
Suite 2320 Rayburn House Office Building
Washington, DC 20515

Attention: Dr. Harlan L. Watson

Dear Congressman Rohrabacher:

In response to your request, enclosed are my answers to the followup questions submitted by Members of the Subcommittee regarding my testimony at the hearing held March 7, 1996, on the Department of Energy's Restructured Fusion Energy Sciences Program.

Sincerely,

William E. Drummond
Professor of Physics

WED:de
Attach.

RESPONSE TO QUESTIONS SUBMITTED BY THE MEMBERS OF THE SUBCOMMITTEE ON ENERGY AND ENVIRONMENT

ON THE TESTIMONY OF PROF. WILLIAM E. DRUMMOND

1) **Q.** Why were these individuals under great pressure "not to do this [basic research on plasma confinement]," and who specifically applied this pressure?

A. What I apparently said sounds more sinister than it is. But, it is a very important point. For the last several years, the DOE has emphasized large development projects such as ITER and TPX as well as"reactor relevant "studies while actively discouraging basic research into fusion physics. Basic scientific issues such as the underlying physics of plasma confinement have not been regarded as "competitive". This perspective has dominated the allocation of DOE funds and even in the national labs, it takes courage for group leaders to endorse fundamental studies by young scientists -- particularly, when these can take 4 or 5 years to come to fruition. Fortunately, the head of the Institute for Fusion Studies at The University of Texas and the head of the Princeton theory group had the courage and wisdom to support this work, and the results already seem to have profoundly changed the fusion program.

Hopefully, the restructuring of the fusion program which FEAC has proposed will in the future encourage such research rather than discourage it .

2) **Q.** Do you agree or disagree with Dr. Conn's assessment "[t]he prospects for an economically viable and environmentally acceptable commercial tokamak electric power plant are very good" and that "[w]ith a rigorous research program, fusion power can produce a substantial portion of U.S. energy needs in the second half of the next century?"

A. Yes! The confinement of fusion plasmas is the central issue in the quest for practical fusion power. New experimental results show both longer confinement and confinement at higher plasma beta. More importantly, we are for the first time beginning to understand the underlying physical phenomena that control plasma confinement and how we can operate experiments to produce even better results. This will take time, but the next several years, if the program is intelligently managed, should be the most fruitful period in the history of the program.

3a) **Q.** Would you please comment on the statements of Professor Stix and his colleagues regarding the ITER-EDA and the EDA effort in building an "Interim Design"? Do you have any diagreement with their statements?

3b) **Q.** One conclusion flowing from the comments of Dr. Stix and his colleagues is that continuing to participate in and to fund the present ITER-EDA path may not be the wisest use of scarce resources. Would you agree, and if not, why do you disagree?

4) **Q.** Given this, [the PCAST], assessment of ITER, why should Congress continue funding the ITER EDA for over two additional years at a cost of at least $55 million per year?

A. I agree with both Professor Stix and the PCAST report. If ITER-EDA is to be supported, it is not for its scientific value but rather for its presumed value as a harbinger of more attractive future international programs. It is argued that if we were to stop our participation in the ITER-EDA that our reputation as a reliable international partner would suffer. As a result, it is argued, this could bar us from future participation in other more scientifically oriented international projects which we might be able to afford and in which we might have a genuine scientific interest.

It is beyond my ken to judge whether these arguments have merit.

5) **Q.** Do you have confidence that the current OFE management has the will and ability to properly implement such a [science] program?

A. No! The fusion program has been run as an industrial development program for the last 20 years, and the DOE staff simply does not have the expertise, experience, or scientific perspective required to manage the science oriented program recommended by FEAC. The OFE should be headed by a distinguished scientist from outside the DOE who has an established record of research in the field, and who has the same priorities as those given by the FEAC and PCAST reports.

6) **Q.** On Page 6 of your prepared testimony, you recommend reducing fusion staff overhead from $9.6 million to $4.5 million -- a 53 percent reduction. What is the basis for your recommendation?

A. The DOE staff was assembled to run a development program. With the change to a scientific research program, the level of the staff should be similar to that of other DOE research programs of similar size, and that would be about a third the level of the present staff.

7) **Q.** In your recommended $212.7 million "Domestic Science Budget" on page 7 of your testimony, you recommend zeroing out both the MFE Computing and Inertial Fusion budget line items. What is the rationale for these recommendations?

A. These two budget items are anachronisms.

The MFE computing center was initially set up by the fusion program, but it was converted to a DOE-wide center many years ago. However, for historic reasons, part of the support for this center continued to be charged to fusion. I am informed that this is no longer true, and that the FY97 budget request reflects this.

The Inertial Fusion line is to support a program which for historic reasons was charged to magnetic fusion but, as Dr Conn testified, is not related to the present fusion program. If this is to be supported, it should be by some other part of the DOE.

University of Illinois
at Urbana-Champaign

**Department of
Nuclear Engineering**

214 Nuclear Engineering
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103 South Goodwin Avenue
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Fusion Studies Laboratory

217 333-3772
217 333-2906 *fax*

4 April 1996

U.S. House of Representatives
Committee on Science
Subcommittee on Energy and Environment
Suite 2320 Rayburn House Office Building
Washington DC 20515-6301

Attention: Dr. Harlan L. Watson

Dear Dr. Watson:

As requested by Congressman Rohrabacher, Chair, Energy and Environment Subcommittee, I am attaching my answers to the additional questions he forwarded to me on 15 March. If I can provide additional information or input for the Subcommittee, please do not hesitate to contact me. I can be reached at 217-333-3772 (vox) and 333-2906 (fax), or *g-miley@uiuc.edu.*

Sincerely,

George Miley

George H. Miley, Director
FUSION STUDIES LABORATORY

GHM/ce
Enclosure (1)
Attachment (1)

cc: Congressman Dana Rohrabacher
Congressman Harris W. Fawell

Response to Follow-Up Questions from the Subcommittee on Energy and Environment

The responses below follow the numbered questions from the Subcommittee, as listed in *Attachment A*.

1. I strongly disagree with Dr. Conn's assessment that "the prospects for an economically viable and environmentally acceptable tokamak electric power plant are **very good**." There is no concrete evidence to support his claim. Thus far, the extensive reactor design studies carried out under sponsorship of the Office of Fusion Energy have, at best, produced a vision of a fusion power plant that is characterized by marginal economics and debatable environmental advantages. It is for this reason that I recommended in my testimony that we must identify a new goal, **a new vision**. The example I gave, the Japanese ARTEMIS reactor study, employed D-^{3}He fuel in a field-reversed configuration (FRC) reactor to reduce radioactivity, to reduce neutron damage and activation of materials, and to obtain a large fraction of charged-particle fusion products, such that a highly efficient and economically attractive direct conversion system could be incorporated. Other high-β alternate concepts might also achieve this goal, but a tokamak cannot.

   In summary, if the word *tokamak* is removed from Dr. Conn's statement (i.e., if the statement becomes "... prospects for ... acceptable ***fusion*** electric power plants are **very good**."), I would agree. The issue, then, is how to structure a vital program that will achieve this goal in an expeditious and cost-effective manner.

2.a. I agree with the general thesis of the letter by Professor Stix, as published in *Science*. One basic theme is the need to place the highest priority on the maintenance of robust fusion science national programs. The issue he does not address, however, is what national programs should receive priority. For reasons clear in Item #1 above, I stressed in my testimony the need for the U.S. to develop a strong national program in alternate concepts, along with the necessary supporting programs in fundamental fusion science, engineering, and technology. In contrast, the Stix letter seems to imply a first priority on maintaining all of our large facilities, such as Thermonuclear Fusion Test Reactor (TFTR), DIII, etc.--all of which are tokamaks. If so, I strongly disagree with that direction. The tokamak makes an excellent test bed for study of fusion-relevant plasmas, but a balance must be struck between that and a vigorous program in alternate confinement.

2.b. I agree with the thrust of the comments by Dr. Stix and his colleagues that continuing to participate in and to fund the present ITER EDA path is not a wise use of scarce resources. I would not, however, favor an abrupt and hasty termination of U.S. involvement in this program; rather I see it being phased out over the next two years while the alternative and basic programs in the U.S. are expanded. Unless this is done, there is simply not enough money to develop a program that addresses the vision I spoke of in my testimony.

3. The statement quoted on page 20 of the PCAST report is, in a sense, a restatement of the Stix conclusion that funding the ITER EDA at the current level "may not be the wisest use of scarce resources." Thus, my response is the same as given in Item #2.b., namely that we should consider a gradual withdrawal from this program over the next two years. I favor a phased reduction to provide an orderly process for redirecting the efforts of the outstanding

fusion scientists and engineers working on the ITER EDA project, while allowing for a calculated, systematic expansion of the new alternate concept and basic science/engineering programs.

4. My term "unique base technology" relates to the technology issues involved in developing a reactor system, such as illustrated by the ARTEMIS design. Examples of these issues include the new physics of alternate concepts suitable for burning D-^{3}He--examples being large-orbit stabilization of the plasma, high-β operation, open field regions for coupling out a portion of the charged fusion products--issues not presently addressed in large, low-β tokamak plasmas. With the reduced neutron flux, hence reduced neutron damage, the whole basic materials program must be rethought, since the selection of optimal materials will be quite different from that needed for high-neutron-flux D-T systems. Rather than handling neutron fluxes, the emphasis shifts to particle and radiation flows, heat loads on the reactor and direct converter walls, etc. Finally, as shown by the ARTEMIS example, the energy conversion system is an essential component in a fusion power plant. In ARTEMIS, the advanced "traveling wave converter" provided the high efficiency and was a key element leading to a low cost of electricity. Thus an advanced energy conversion technology must be incorporated into the technology program, if such a reactor vision is to be achieved.

5. I stated that the "root problems" remain largely ignored in the current FEAC plan, because the question "Is the present goal of the DOE program the proper one?" has not been addressed. Had the FEAC Committee done this, and had they reached the inevitable conclusion that I and many others have reached, i.e. that the goal needs to be changed (a **new vision** is needed), they would have been forced to consider how to revise the program to achieve the new goal. What they have done is merely to propose a revised program to reach the same questionable goal. Consequently, they did not tackle the root problems, which keep us fixated on the tokamak reactor goal, despite the fact that, as shown by the the Stix and colleagues letter in Item #3, the FEAC Report itself as quoted in Item #4 above, and my written testimony, grave doubts have been raised about the validity of that goal.

Attachment A. (3 pages)

HEARING OF THE SUBCOMMITTEE ON ENERGY AND ENVIRONMENT
COMMITTEE ON SCIENCE
U.S. HOUSE OF REPRESENTATIVES

on

The Department of Energy's Restructured Fusion Energy Sciences Program

Thursday, March 7, 1996
9:30 am
2318 Rayburn House Office Building

Followup Questions Submitted to

Professor George H. Miley, Director, Fusion Studies Laboratory, University of Illinois

1. One of the questions posed in the hearing charter was the following: "What are the prospects that a tokamak fusion reactor will be an economically-viable and environmentally-acceptable commercial electric power producer, and it what time frame?"

   Dr. Conn has responded that "[t]he prospects for an economically viable and environmentally acceptable commercial tokamak electric power plant are very good" and that "[w]ith a rigorous research program, fusion power can produce a substantial portion of U.S. energy needs in the second half of the next century."

   Do you agree or disagree with Dr. Conn's assessment "[t]he prospects for an economically viable and environmentally acceptable commercial tokamak electric power plant are very good" and why?

2. A letter published in the February 16, 1996, issue of *Science*, signed by Professor Thomas H. Stix of Princeton University and cosigned by 8 other prominent members of the fusion community—including Dr. Harold P. Furth, former Director of Princeton Plasma Physics Laboratory—stated the following with respect to the International Thermonuclear Experimental Reactor (ITER) Engineering Design Activities (EDA) and the EDA effort in producing an "Interim Design", which details and costs an engineering test reactor based on the ITER 1991 Conceptual Design Report:

   "At present, it is not known how to construct a fusion reactor economically. To enter directly into the actual construction of the Interim Design—a single machine of grandiose scale and cost in time, human effort and money—suggests otherwise and would establish a commitment to a highly specific direction of development from which it would prove increasingly difficult and embarrassing to depart.

   "The Interim Design would take four giant steps simultaneously, each of them untested: Huge size, ignition, long pulse with fusion-grade plasmas, and very large volume superconducting magnets operating at their ultimate magnetic fields. To focus the world's fusion research efforts on the construction of this machine, which would not produce its first plasma until 2008 or even later, would constitute a high-risk choice.

"The stated target for the Interim Design was to achieve ignition and remain in a state of equilibrium burn for 1000 seconds while producing 1500 megawatts of fusion power. Experimental data and theoretical understanding do not support the achievement of this performance under the proposed operating regime. Separate and serious concerns have been raised that the actual plasma and fusion performance in this machine would not even reach the fallback target, significant long-term power amplification. Such a failure of an internationally sponsored would be a humiliating setback for the entire fusion world.

"The EDA has already defined the Interim Design sufficiently, and a broad-based independent assessment could be made now, rather than after 1998.

"The ITER Council and the EDA leadership have not given serious attention to strategic alternatives for international collaboration, other than moving along doggedly into the construction of the interim design.

"The learning curve in fusion science is steep. For example, great advances in experiment, theory, and computation have occurred in the past 5 years. With support, this rate of progress may be expected to continue. The speed of major device construction should be scaled to the appearance of new ideas and fundamental improvements (and we should use history as a guide).

"We place the highest priority in fusion research on the maintenance of robust national programs [including the multinational Joint European Torus (JET)]. Scientific advances of enormous potential value to economical fusion power have grown out of these programs. Efforts that use existing facilities, or those that can be built in the near future, would be our best use of timer and money. We suggest that the ITER council start immediately to identify new strategies to give us well-targeted and cost-effective ways to advance the world's quest for this enormously desirable energy source."

a. Would you please comment on these statements and tell us if you have any disagreements with Professor Stix and his colleagues?

b. One conclusion flowing from the comments of Dr. Stix and his colleagues is that continuing to participate in and to fund the present ITER EDA path may not be the wisest use of scarce resources. Would you agree, and if not, why do you disagree?

3. On page 20 of the President's Committee of Advisors on Science and Technology (PCAST) Fusion Review Panel's report issued in July, 1995, it is stated that ""by relying on a conservative approach to confinement physics, ITER will (at best) end up demonstrating an approach to fusion that is unlikely to be extrapolatable to an attractive

commercial reactor—the power density in this approach would be low, the physical size of the plant and hence the cost of construction and maintenance would be high, and the minimum practical unit size would be so large as to restrict interest in such plants to only the largest electric utilities; a demonstration of this approach, even if completely successful in physics and technology-development terms, could be a setback for fusion's prospects of commercialization unless accompanied by parallel demonstrations of approaches likely to lead to more compact, more economical reactors."

Given this assessment of ITER, why should Congress continue funding the ITER EDA for over two additional years at a cost of at least $55 million per year?

4. In your summary on page 1 of your prepared testimony you state that "[a] generally accepted goal is the development of an economically competitive fusion reactor that has maximum environmental compatibility and safety features. Based on what we know today, that goal could only be achieved by a radical departure from mainline concepts, e.g., by a D-^{3}HE-fueled, high-β system, emphasizing direct energy conversion. Yet neither such systems nor the unique base technology needed to achieve them are currently part of the DOE plan."

   Could you please elaborate on what you mean by the term "unique base technology" that is not part of the current DOE plan?

5. On page 3 of your prepared testimony you state that "the root problems remain largely ignored in the current [FEAC] plan." Would you please elaborate on this statement?

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April 4, 1996

The Honorable Dana Rohrabacher
Chairman, Subcommittee on Energy and Environment
c/o Dr Harlan L. Watson
B374 Rayburn House Office Building,
Washington, DC 20515

Dear Congressman Rohrabacher:

The Department of Energy's Restructured Fusion Energy Sciences Program: Follow-up Questions

Thank you for your letter of March 15, 1996, and for the opportunity to respond to your follow-up questions submitted in connection with the above hearing. My responses are below:

1. Do you agree [that] [t]he prospects for an economically viable and environmentally acceptable fusion commercial tokamak electric power plant are very good and why?

Based on our achievements in tokamak research to date, I believe that this route to fusion could conceivably produce a functioning power reactor. However, I do not believe that the prospects for an economically viable commercial plant can be classed as *"very"* good. Relative to its potential competitors in the future energy marketplace, a conventional tokamak reactor has the following perceived disadvantages:

- High capital cost due to the multiplicity of large, complex systems necessary to make it function.
- High complexity and, therefore, low perceived reliability of hard-to-maintain, high technology hardware.
- Large unit sizes in excess of 1000MW(electric) to realize even marginal economics from economy-of-scale considerations (see also comments on this aspect under questions #4 and #6).
- High development costs (>$10B) for the next, proof-of-principle stage, notwithstanding that the commercial viability of the ultimate power reactor may appear questionable.

Despite that, only the tokamak now allows us to study the physics of confined plasmas levels approaching those needed for a reactor. Additionally, a sustained fusion burn from a mixture of deuterium-tritium fuel can conceivably be achieved in the tokamak. It can be improved significantly for higher performance and sustained (rather than pulsed) power output, and for this reason I support continuing research on the tokamak as a plasma physics research tool within a diverse mix of concepts. However, since its ultimate reactor viability is by no means assured, I strongly suggest we should not be devoting the vast majority of our fusion research dollars to this route alone. I believe it is essential to strive for about 25% of the fusion budget to be directed to alternative approaches to fusion energy and, in particular, to those concepts whose reactor embodiments have the potential of alleviating the perceived reactor problems listed above.

2. Would you please comment on [the statements of Professor Stix and his colleagues regarding the desirability or otherwise of continuing with the present direction of the International Thermonuclear Experimental Reactor (ITER) EDA into the construction phase]

Like Professor Stix *et al.* , as suggested by PCAST, and by the Program Directors of the major US fusion institutions in their letter to PCAST, I would argue for a different approach in international fusion development.

As a member of the original international design team for ITER during the Conceptual Design Activities (CDA), 1988-91, I fully support the desirability of a vigorous, international approach to research in fusion power development. However, I believe this international collaboration should not be focused on just one large machine of questionable merits, but on a process in which, by consensus agreement, we coordinate a strategic approach to an attractive reactor product. I do not believe that ITER enjoys the support of the majority of the scientific community in the US.

At the end of the CDA, I submit that we knew quite well the type of product that would result from an ITER construction project and the detailed engineering work of the EDA has only confirmed that vision. While we no longer have ambitions to be an equal partner in this endeavor, we would nonetheless be party to the construction of a conventional tokamak that would consume a significant portion of the presently-envisaged fusion budget. I personally do not believe this should be done at the expense of all other routes especially fusion alternatives.

We should note that ITER is a verification experiment for tokamak ignition, i.e., it's the most cost effective machine that we believe can be built to verify long pulse ignition in a tokamak, given today's conservative database. However, even if ITER passes its verification tests perfectly, then we still would have no viable commercial fusion reactor prospect. The best thing that ITER will do is work as expected and, thereby, verify 1980's-vintage physics. It will take ~15-20 years to confirm what we know on paper today.

Finally, let me underline one point that Dr Stix and colleagues made in their *Science* letter, i.e. that entering the construction phase of ITER would *"....establish a commitment to a highly specific direction of development from which it would prove increasingly difficult and embarrassing to depart".* In this context, even if ITER passes its verification tests, it runs the real risk of prejudicing the world view of the ultimate potential of fusion energy. Prospective customers (utilities, power companies, investors, etc.) will draw conclusions, from the size, cost, and complexity of this machine, that fusion is just not attractive. We should instead focus on increasing the attractiveness by research on smaller and less costly innovative devices.

3. Given [the PCAST] assessment of ITER [prospects], why should Congress continue funding the ITER EDA for over two additional years at a cost of at least $55 million per year?

While I agree with the remarks of Professor Stix *et al*, and also concur with the PCAST assessment, there may be two reasons why the U.S may wish to continue with the ITER EDA for two further years:

(1) First, we have signed an international agreement to commit personnel and resources to the ITER project until the end of the present EDA (1998), and have agreed that fusion power plant development should proceed in an international arena. In view of our reputation in future similar international ventures, it may be advantageous to keep to this agreement, albeit at a reduced budget level, even though it may not be in the best interests of fusion development. If we do continue with the EDA for two further years, we should devote some portion of the remaining period to serious international negotiations for alternative strategic directions. It is my hope that the parties will determine that different next step(s) are more beneficial to fusion development.

(2) Second, it is possible that one or more countries may ultimately offer to shoulder virtually all the financial burden of ITER construction with only token contributions from other "junior partners" such as the U.S. This could be an attractive leveraged investment for the U.S, and that should be debated. If, however, our token contribution is funding that would otherwise go to alternative fusion research then I would personally not support this route.

Note that, in any event, I would commend the present ITER team in their performance of excellent scientific and technical work and in forging links that will be of great benefit to future international collaboration.

4. There have been a number of conceptual reactor design studies which purport to show cost-of-electricity projections for tokamak power reactors roughly a factor of two or so larger than current fission reactors. What are the uncertainty in this projections and how large are they?

The three most important uncertainties are:

(1) The uncertainty that the physics and technology will work as projected on paper today (Approximately a factor of two uncertainty in the technology and somewhat more in the physics).

(2) The uncertainty in the capital cost of the extrapolated technology (Approximately a factor of two; probably more for the advanced technology for which no prototypes exist today)

(3) The issue of complexity, reliability, maintainability and, therefore, the uncertainty in the availability of the tokamak power core (Uncertainty in availability has never been formally quantified due to the lack of an operating database; however, see comment below)

The first of these will be reduced by further research, so ultimately we would know whether the physics and technology worked before committing to commercial construction. Uncertainties in the physics at least can play both ways, and it is conceivable that with future investment in research, we may discover new physics operating regimes which improve the operational performance of the tokamak.

The uncertainty surrounding projected reliability and availability is large. For example, an ITER maintenance study was performed in 1991. This indicated that, in the event of a serious failure of a toroidal magnetic field magnet, it would take in the range 3-5 years to replace this magnet once the machine had been operating for 1 year. There is no indication that a commercial tokamak reactor would be substantially less complex than ITER. Thus the possible perception by utilities that a tokamak fusion reactor is not maintainable in a finite time frame suggests that they might be considered non-viable on this aspect alone.

Economy-of-scale arguments suggest that tokamak reactors in unit sizes significantly larger than $1GW_e$ (i.e. significantly larger than one billion Watts electric) may approach a competitive cost-of-electricity, but the problem of complexity/reliability surely negates that conclusion. While the utility structure in the next century may be different from today, and multi-GW electricity reservations may become the norm, it must be considered questionable that any utility -- private, public or government-owned -- will invest in a large, single heat source which is perceived to be vulnerable to frequent outages and significant downtimes for repair. Hydroelectric plants are commonly found in multi-GW(electric) sizes. However, these have the crucial difference of redundancy, i.e., each plant has the modularity of parallel water feeds to a number of independent turbine-generator sets. Note, that conceptual fusion reactors based on inertial confinement fusion (ICF) do suggest the potential for redundancy and this is one major advantage of this class of concepts (see below).

5. If the tokamak is not an attractive fusion reactor, of what value is (a) continued work on ITER and (b) continued operation of large tokamak facilities?

(a) See answer to question #3 above

(b) While I believe that the tokamak, by itself, is unlikely to lead to fully attractive commercial reactor, I believe that the tokamak should be viewed as a valuable step towards the ultimate goal of economic and competitive fusion energy. This view is based on the tokamak's role, not yet completed, as a tool for developing the fusion science of high temperature plasmas. Because of the tokamak's capacity for holding heat and its effectiveness in achieving the required magnetic field configuration, it has been the best research tool for achieving fusion conditions in the laboratory so far. Also, as I said above, from our understanding of today's database, the tokamak could conceivably produce a functioning power reactor, albeit not necessarily a very attractive one. Thus as a scientific research tool for high temperature plasmas, I support continuing research on the tokamak as a plasma physics research tool within a diverse mix of other concepts. However, I suggest that we should not be spending the vast majority of our fusion research dollars on this route alone.

6. What are the reactor prospects of [Inertial Confinement Fusion] ICF?

ICF provides a route to a power plant embodiment which is fundamentally different in many respects from that of a tokamak and other fusion concepts of the tokamak class such as the stellarator, spherical tokamak and reversed field pinch. There are no large, expensive superconducting magnets that are exposed to potentially damaging fusion radiation in ICF reactors. This unique feature allows lifetime fusion chambers to be designed with renewable liquid coolants facing the targets, instead of solid, vacuum-tight walls that can damage due to the fusion heat and radiation. This permits use of low-activation coolants, and may eliminate the need for billion-dollar neutron sources for solid materials development. The most expensive single component of an ICF plant is the driver, but this isolated from the nuclear-grade components of the fusion chamber. This separation allows a single driver to operate several fusion chambers through a beam switchyard. This modular, multiple-unit approach to IFE power plant design will promote flexibly-staged utility plant deployment and, most importantly, provides the crucial requirement of redundancy (see comment in #4 above regarding the need for redundancy in plants with large electrical output). It is believed that for these and associated reasons, fusion reactors based on ICF will demonstrate a lower cost-of-electricity than those based on the tokamak.

ICF is also a relatively new approach to fusion compared to the tokamak. It is a growing energy research area in Europe and Japan, but it is still an international field in which the U.S is the clear world leader. Development of much of the science basis and the targets (the small fuel capsules that would generate the fusion energy in small microexplosions) will be carried out by defense programs for science related to stockpile stewardship. This is because inertial fusion ignition research is dual-use for energy as well as defense. Complimentary energy research funding can then concentrate on remaining needs specific to the fusion power plant (e.g., higher efficiency and pulse-rate laser or ion beam drivers for power plants). Also, the fact the plasma confinement systems in ICF are small targets the size of a pea means that redesign and testing of different ideas to evolve and innovate ICF confinement in the research stage can be done in rapid iterations and at relatively low cost. For these reasons and the paradigm shift in the reactor embodiment, I suggest that ICF should rightly be considered as *the* primary alternative within a diverse mix of alternative fusion concepts. It is certainly deserving of an increased budget under an expanded alternatives program.

7. [Y]ou cite the stellarator as an example of an alternative concept that has no better reactor potential than the conventional tokamak. What is an example of an alternative concept whose reactor potential exceeds that of the conventional tokamak.

To answer this question I refer you to Table 1 attached which provides a reasonably complete list of fusion concepts which are, or have been, under study at some level in the world program and classifies them according to their main operating principles. Under the first class ("Low Density Magnetic Confinement"), concepts near the top of the list have a better reactor potential than those near the bottom, providing the physics basis can be substantiated. This, in my opinion is one area where research dollars on alternative fusion concepts should be invested. In particular the "spheromak" and the "field-reversed configuration" are characterized by the fact that the magnetic fields that confine the plasma are generated mainly by currents flowing in the plasma itself rather than in large, external magnet systems. This makes for a simpler, cheaper reactor core. Both concepts have undergone considerable past research but are not supported significantly today. Their successful implementation as reactors will require that their physics basis is fundamentally understood. Because of their reactor potential, both concepts are deserving of funding under an expanded alternatives program.

The reactor advantages of the second class of concepts ("Inertial Confinement Fusion") were discussed above under question #6. Here, I strongly recommend that we leverage the investment of the defense-related ICF programs with a strong, complementary program in the power-plant-related aspects, particularly high efficiency drivers and advanced, high gain target concepts.

The third and fourth classes of fusion concepts in Table 1 are interesting in that they represent very different physics operating regimes from that of the tokamak. Many are at a rudimentary stage of exploration and are examples of high risk, high payoff areas that, crucially, would be very cheap to explore to the next stage. However, at present there is virtually no funded work in these areas in the U.S fusion program. Here are examples of the need to the promote intellectual stimulation in breadth to encourage parallel approaches with the acknowledgment that perhaps only a very few, if any, may ultimately be successful. However, we only need one!

In closing, I would like to thank you and the Committee for the opportunity to respond to these follow-up questions. As I said in my prepared testimony, fusion is the only indigenous energy source that will last as long as the earth lasts. Because of the profound benefit to future humanity, it is incumbent on us to continue with an innovative and, most importantly, diverse research program until that goal is accomplished. I suggest it is too early -- and unnecessary -- to put all our eggs in one basket at this stage of fusion development. Therefore, I believe it is essential to strive for about 25% of the fusion budget to be directed to alternative approaches to fusion energy.

Sincerely,

L. John Perkins
Magnetic Fusion Energy Program
Lawrence Livermore National Laboratory

Table 1. CLASSIFICATION OF ALTERNATIVE FUSION CONCEPTS

LOW DENSITY MAGNETIC CONFINEMENT:

- Standard field-reversed configuration
- Large-orbit field-reversed configuration
- Spheromak
- Spherical tokamak
- Reversed field pinch
- Conventional and advanced tokamak
- Stellarator
- Mirror

INERTIAL CONFINEMENT FUSION:

- Standard inertial fusion (heavy-ion-driven, laser-driven, ...)
- Advanced, fast-ignition systems
- Magneto-inertial concepts
- High-yield pulsed systems

HIGH DENSITY MAGNETIC CONFINEMENT:

- Pulsed Z-pinches (fiber, laser-assisted, staged pinches, ...)
- Plasma foci
- Continuous flow pinches
- Wall-confined, magnetically-insulated concepts

NON-THERMONUCLEAR AND MICELLANEOUS:

- Inertial electrostatic confinement
- Colliding beam systems
- Coulomb barrier circumvention concepts

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April 16, 1996

Mr. Harlan Watson
Subcommittee on Energy and Environment
Committee on Science
B374 Rayburn House Office Building
U.S. House of Representatives
Washington, DC 20515

Dear Harlan,

Enclosed are my answers to the followup questions from the hearing before the Subcommittee on March 7, 1996.

If you have any questions, please feel free to contact me.

Sincerely,

Rob W Conn

Robert W. Conn
Dean

Enclosure

Questions and Answers to Hearing of the Subcommittee on Energy and Environment
Committee on Science
U.S. House of Representatives
Thursday, March 7, 1996

Followup Questions and Answers from
Robert W. Conn

1. Dr. Conn, you were the Vice Chair of the PCAST Fusion Review Panel, which issued a report in July, 1995. On page 5 of your prepared testimony, you summarize briefly the similarities between the PCAST and FEAC recommendations. Could you also please summarize the differences between the PCAST and FEAC recommendations?

The main differences between the recommendations of the PCAST Fusion Review Panel and the Fusion Energy Advisory Committee are in the level of funding recommended and the specific degree of participation in the world fusion program. Again, both FEAC and PCAST recommend a science and basic technology focus for the U.S. program, recommend obtaining the maximum scientific benefit from our existing facilities, recommend increasing the amount of work on advanced tokamak and alternative magnetic confinement research, recommended pursing work on low activation materials and basic technologies needed for success of the confinement experimental program, recommend a specific completion date for the scientific program of TFTR, and recommend removing a target date of 2025 for the demonstration of practical fusion power in a specific device.

Specifically as to differences, PCAST recommended a stable budget at $320 million per year and recommended that the U.S. discuss the possibility of cooperating world-wide on a lower cost version of an experiment focused primarily on ignition and moderately long pulse operation. It specifically suggested that the U.S. be prepared to offer $1 billion over 10 years (averaging $100M per year) as our contribution to such a project. PCAST's recommended position is that if the parties decide to build the ITER as it is presently designed, that would also achieve the objectives of the U.S. program, but that our financial obligation should not exceed $1 Billion. FEAC on the other recommended a stable budget level of $275 million, and did not include funds to participate in the ITER program above about the present level of funding, i.e., between $55 and $60 million per year. It recommended sustaining our commitment to the ITER engineering design activity (EDA) through the end of it phase in 1998. FEAC also recommend funds for basic plasma physics at up to 5% of the program budget as its stewardship responsibility for the field, and noted that high temperature plasma physics, as the largest part of this important area of scientific research, is the focus of the bulk of the resources in the restructure program it recommended.

2. On page 20 of the President's Committee of Advisors on Science and Technology (PCAST) Fusion Review Panel's rep issued in July, 1995, it is stated that "by relying on a conservative approach to confinement physics, ITER will (at best) end up demonstrating an approach to fusion that is unlikely to be extrapolatable to an attractive commercial reactor--the power density in this approach would be low, the physical size of the plant and hence the cost of construction and maintenance would be high, and the minimum practical unit size would be so large as to restrict interest in such plants to only the largest electrical utilities; a demonstration of this approach, even if completely successful in physics and technology-development terms, could be a setback for fusion's prospects of commercialization unless accompanied by parallel demonstrations of approaches likely to lead to more compact, more economical reactors."

Given this assessment of ITER, why should Congress continue funding the ITER EDA for over two additional years at a cost of at least $55 million per year?

FEAC recommended funding the ITER project for the duration of the EDA activity because ITER is an experimental machine that, if built, will produce ignited and very long pulse or steady state burning plasmas in a configuration fully capable of investigation the full complement of technologies needed in almost any likely fusion power station. Whether or not ITER itself is exactly what a final fusion power station looks like is less important than the capability of the device to explore fully both the physics of burning plasmas and the technologies needed for practical fusion power. Given the capability of the machine to explore these issues in depth, both FEAC and PCAST came to the conclusion I have explained. It is my own view, and I believe it is reflective of the view of FEAC, that if ITER is built with funding primarily from one party, and that the U.S. could participate for the cost of our present commitment, or even a bit more, the investment relative to the return in knowledge and experience, would be an exceptional one.

There is also the ethical point. We have basically contracted with our international partners to provide support for the ITER EDA, and our partners have said they want to complete the EDA program. Some have even indicated their willingness to provide the bulk of the resources to build ITER. In their view, ITER as designed in close to a power station design, and since that is a matter of debate, my view is that we have a moral obligation to meet our contractual obligations to our international partners. I have explained in the first part of my answer that I also believe the are very strong technical reasons for doing so, and when the two arguments are taken together, the result is the recommendation provided by both PCAST and FEAC.

3. Why did both the President's Committee of Advisors on Science and Technology (PCAST) and FEAC give so little attention to inertial confinement fusion (ICF)?

Inertial confinement fusion (ICF) is a defense program, not an energy or science program, and is support in DOE by the Office of Defense Programs. Further, much of that program is classified and is not as easily or as openly reviewed. The questions about the "fusion program" raised in Congress were about the program of the Office of Fusion Energy within the Office of Energy Research at DOE. The OFE program has had the mission of supporting research on fusion energy, has been an unclassified program, and has focused on magnetic confinement approaches to fusion energy. The only program in OFE related to ICF is that to develop a heavy ion accelerator with characteristics appropriate to implode ICF targets of a classified design. Accelerator development is done in many programs in DOE, including high energy physics, nuclear physics, and basic energy sciences. It was the judgment of both PCAST and FEAC that ICF is distinct as a program from that of the fusion energy program, that the review committees were not set up with the appropriate expertise or time frame to review the program effectively, and that without a review of the program in DP, one could not review the heavy ion program and determine readily its role in OFE. The heavy ion accelerator program has in fact been reviewed several times in recent years, and the only question FEAC discussed was whether OFE, as opposed to OBES, DP, or another Office at DOE, is the most appropriate home. FEAC did not have time to address or answer that question.

4. A letter published in the February 16, 1996 issue of *Science*, signed by Professor Thomas H. Stix of Princeton University and cosigned by 8 other prominent members of the fusion community--including Dr. Harold P. Furth, former Director of Princeton Plasma Physics Laboratory--stated the following with respect to the International Thermonuclear Experimental Reactor (ITER) Engineering Design Activities (EDA) and the EDA effort in producing an "Interim Design", which details and costs an engineering test reactor based on the ITER 1991 Conceptual Design Report:

"At present, it is not known how to construct a fusion reactor economically. To enter directly into the actual construction of the Interim Design—a single machine of grandiose scale and cost in time, human effort and money—suggests otherwise and would establish a component to a highly specific direction of development from which it would prove increasingly difficult and embarassing to depart.

"The Interim Design would take four giant steps simultaneously, each of them untested: Huge size, ignition, long pulse with fusion-grade plasmas, and very large volunle superconducting magnets operating at their ultunate magnetic fields. To focus the world's fusion research efforts on the construction of this machine, which would not produce its first plasma until 2008 or even later, would constitute a high-risk choice

"The stated target for the Intenm Design was to achieve ignition and remain in a state of equilibrium burn for 1000 seconds while producing 1500 megawatts of fusion power. experimental data and theoretical understanding do not support the achievement of this performance under the proposed operating regime. Separate and serious concerns have been raised that the actual plasma and fusion performance in this machine would not even reach the fallback target, significant long-term power amplification. Such a failure of an internationally sponsored would be a humiliating setback for the entire fusion world.

"The EDA has already defined the Interim Design sufficiently, and a broad-based independent assessment could be made now, rather than after 1998.

"The ITER Council and the EDA leadersbip have not given serious attention to strategic alternatives for international collaboration, other than moving along doggedly into the construction of the interim design.

"The learning curve in fusion science is steep. For example, great advances in experiment, theory, and computation have occurred in the past 5 years. With support, this rate of progress may be expected to continue. The speed of major device construction should be scaled to the appearance of new ideas and fundamental improvements (and we should use history as a guide).

"We place the highest priority in fusion research on the maintenance of robust national programs [including the multinational Joint European Torus (JET)]. Scientific advances of enormous potential value to economical fusion power have grown out of these programs. Efforts that use existing facilities, or those that can be built in the near future, would be our best use of timer and money. We suggest that the ITER council start immediately to identify new strategies to give us well-targeted and cost-effective ways to advance the world's quest for this enormously desirable energy source."

a. Would you please comment on these statements and tell us if you have any disagreements with Professor Stix and his colleagues?

I do not agree with my distinguished colleagues on many of the points in their letter to Science. Many issues are addressed in a number of letters sent in response to Science by other colleagues such as Paul Rutherford of Princeton and Charles Baker of U.C., San Diego to Science. I refer you to those letters.

b. One conclusion flowing from the comments of Dr. Stix and his colleagues is that continuing to participate in and to fund the present ITER EDA path may not be the wisest use of scarce resources. Would you agree, and if not, why do you disagree?

As for part b of your question, I do not agree that participating in the ITER EDA for the next two years is an unwise use of scarce resources, and have given the reasons why in the answer to question 3. I also have serious disagreements about the group's assessment of the scientific performance capabilities of the ITER machine if it were built, and my assessment is that if it is built and operates, all my colleagues who signed that letter, including Dr. Stix, will then be defending the operation of the machine as scientifically meritorious, in the same manner that they now defend the operation of TFTR and JET.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 1: On page 5 of your prepared testimony you state that "over the next five years, fusion program funding for basic plasma science is projected to grow to approximately $10 million. This initiative will be coordinated with other agencies funding plasma science, such as the National Aeronautics and Space Administration and the National Science Foundation."

How will the coordination among the various funding agencies take place?

Answer: The coordination activities associated with implementing a basic plasma science initiative will be carried out by meetings between appropriate program officials in relevant government agencies. Key individuals will be identified and coordinating groups established. The first of such meetings took place March 7, 1996, between DOE and NSF. An interagency meeting of plasma physics program managers involving DOE, NSF, and other agencies is being organized. Planning activities will be scheduled throughout the remainder of FY 1996

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 2: On page 5 of your prepared testimony you also state that one of the small alternative concept experiments that has already been favorably reviewed is the National Spherical Tokamak Experiment proposed by Oak Ridge National Laboratory, Princeton Plasma Physics Laboratory, and the University of Wisconsin to be built at Princeton Plasma Physics Laboratory.

a. Why is a spherical tokamak considered an alternative to the conventional tokamak?

Answer: The spherical tokamak is considered an alternative to the conventional tokamak because it has no central magnet and its operating parameters are sufficiently outside the range of the conventional tokamak.

b. What are the potential advantages of a spherical tokamak relative to a conventional tokamak?

Answer: The potential advantages of the spherical tokamak relative to a conventional tokamak are simpler design, with no central magnet and lower magnetic fields, and higher power density inside the vacuum chamber.

c. What is the estimated cost of the experiment?

Answer: The estimated fabrication cost of the proposed spherical tokamak is $18.5 million if located at Princeton Plasma Physics Laboratory where about $50M of available hardware will be re-used.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 3: On page 7 of your prepared testimony you state that "the Department is beginning an interagency process that would lead to our participation in non-committal and informal discussions with our partners about ITER construction."

a. Which agencies are involved in this process?

Answer: a. The agencies involved with the decision-making in this process are the Department of Energy, the Department of State, the President's Office of Management and Budget and the President's Office of Science and Technology Policy.

b. When is the process expected to be completed?

Answer: b. We expect the process to be completed in time for a decision by July 1996 when these exploratory discussions should begin, if a hiatus at the end of the current activities in July 1998 is to be avoided.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 4: On page 7 of your prepared testimony you also state that "[t]he Office of Fusion Energy will be reduced in size and reorganized."

a. What is the anticipated reduction in size?

Answer. a. We expect the number of employees in the Office of Fusion Energy to decrease from the current level of 40 to about 29. The Office had as many as 47 employees in early 1995.

b. When is this reduction and reorganization expected to take place?

Answer. b. The reduction in employment should be achieved by the end of FY 1997. The reorganization of the Office should be completed by this summer.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 5: On page 8 of your prepared testimony you state that "[w]hile the broad outlines of the program are clear, many important decisions remain."

What are the remaining important decisions?

Answer: The important decisions remaining to be made in restructuring the fusion energy sciences program are:

a. Considering only existing facilities, what is the optimum mix of these facilities and their operations time during the next 1-5 years? A specific part of this question is how long should experimental operations on the TFTR continue before shutting it down so that the funding can be made available for other efforts?

b. What alternative concept research investment strategy should be pursued in order to maximize the science gained at minimum cost?

c. What is the US role going to be in any international agreement on the construction, operation, and decommissioning of an International Thermonuclear Experimental Reactor.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 6: Dr. C. Paul Robinson, on pages 9 and 10 of his March 12, 1996, testimony before a Joint Hearing of the Subcommittee on Military Procurement and Subcommittee on Research and Development of the House National Security Committee, stated the following:

"I am concerned that some recent policy discussions regarding the content of a comprehensive test ban treaty could restrict the laboratories' ability to conduct an adequate stockpile steward in the absence of testing. If the Arms Control and Disarmament Agency decides to seek provisions in a test ban treaty that restrict inertial confinement fusion (ICF) to only laser and particle beam drivers, based on the negotiating record of the 1975 Nonproliferation Treaty, then other methods of driving ICF, some of which are being aggressively developed here and abroad, would be treaty violations."

a. Do any of these "other methods of driving ICF, some of which are being aggressively developed here and abroad" have potential to be drivers for ICF energy applications?

Answer a. Reviews of energy applications of inertial confinement fusion to date have only substantiated the potential of laser and ion beam driver approaches. However, the possibility of other driver approaches developing from scientific exploration cannot be discounted.

b. What is the Department's position with respect to the comprehensive test ban treaty ICF driver issue?

Answer: b. The Department is currently participating in discussions within the Administration on this and a variety of Comprehensive Test Ban Treaty

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 7: How might defense applications of ICF affect the pace of progress in developing its civilian use?

Answer: The IFE program is dependent upon the Defense Programs funded Inertial Confinement Fusion program for the fundamental target ignition and burn physics. For the foreseeable future, the pace of inertial fusion energy development is limited both by the pace of obtaining ignition information within the Defense Programs funded efforts and by the funding available within the fusion energy program to support enabling technology development.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 8:	What are the estimated FY 1998, FY 1999, FY 2000, FY 2001 and FY 2002 budget authorization funding requirements, by Fusion Energy subprogram, given the FY 1997 budget request for Fusion Energy?
Answer:	Estimates by subprogram for the outyears are not available. However, the Department has endorsed the recommendation of the recent Fusion Energy Advisory Committee Report, "A Restructured Fusion Energy Sciences Program," that recommended a restructured fusion program. With stable funding at the FY 1997 request level in FY 1998 and beyond, we could pursue a balanced FEAC program in plasma science, concept innovation, and international collaboration by maintaining world leadership in selected areas of expertise. The Department recognizes the importance of stable funding for fusion and to the extent possible will try to maintain a constant level of funding with no significant increases planned above the FY 1997 level. One of the changes underlining the restructured program was that there are no large project mortgages included in the budget The Tokamak Physics Experiment construction project that was previously planned for has been terminated.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 9: What is the level of funding for each DOE Laboratory, by Fusion Energy program/subprogram, for each of FY 1995, FY 1996 and 1997?

Answer:

Fusion Energy Sciences
(B/A In Thousands)

Laboratories	FY 1995	FY 1996	FY 1997
Fusion & Plasma Science			
Argonne National Laboratory	302	400	0
Idaho National Engineering Laboratory	178	100	100
Lawrence Berkeley Laboratory	135	70	75
Lawrence Livermore National Laboratory	6,987	4,548	4,810
Los Alamos National Laboratory	2,654	1,600	1,460
Oak Ridge National Laboratory	12,593	7,149	7,275
Princeton Plasma Physics Laboratory	72,859	56,194	59,040
Sandia National Laboratory	713	150	160
Total Fusion & Plasma Science	96,421	70,211	72,920
Fusion Technologies			
Argonne National Laboratory	6,754	4,525	4,000
Brookhaven National Laboratory	83	97	97
Continuous Electron Beam Accelerator Fac	0	5	0
Idaho National Engineering Laboratory	2,935	2,632	2,223
Lawrence Berkeley Laboratory	5,631	4,710	4,930
Lawrence Livermore National Laboratory	8,820	4,745	3,649
Los Alamos National Laboratory	4,181	2,697	2,255
Oak Ridge National Laboratory	14,275	12,078	11,775
Pacific Northwest Laboratory	2,493	1,542	1,680
Princeton Plasma Physics Laboratory	43,030	2,997	2,520
Sandia National Laboratory	7,105	5,573	5,400
Savannah River Laboratory	378	282	400
Stanford Linear Accelerator Center	50	50	50
Total Fusion Technologies	95,735	41,933	38,979

Fusion Energy Sciences
(B/A In Thousands)

Laboratories	FY 1995	FY 1996	FY 1997
Related Capital Funding			
Argonne National Laboratory	687	100	70
Idaho National Engineering Laboratory	110	0	0
Lawrence Berkeley Laboratory	300	300	300
Lawrence Livermore National Laboratory355	100	200	
Los Alamos National Laboratory	315	0	0
Oak Ridge National Laboratory	995	250	250
Pacific Northwest Laboratory	240	50	50
Princeton Plasma Physics Laboratory	2,837	1,240	6,250
Sandia National Laboratory	390	0	0
Total Related Capital Funding	6,229	2,040	7,120
Total Laboratories			
Argonne National Laboratory	7,743	5,025	4,070
Brookhaven National Laboratory	83	97	97
Continuous Electron Beam Accelerator Fac	0	5	0
Idaho National Engineering Laboratory	3,223	2,732	2,323
Lawrence Berkeley Laboratory	6,066	5,080	5,305
Lawrence Livermore National Laboratory	16,162	9,393	8,659
Los Alamos National Laboratory	7,150	4,297	3,715
Oak Ridge National Laboratory	27,863	19,477	19,300
Pacific Northwest Laboratory	2,733	1,592	1,730
Princeton Plasma Physics Laboratory	118,726	60,431	67,810
Sandia National Laboratory	8,208	5,723	5,560
Savannah River Laboratory	378	282	400
Stanford Linear Accelerator Center	50	50	50
Total Laboratories	198,385	114,184	119,019

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 10: What is the level of funding, by Fusion Energy subprogram, for each of the TFTR, TPX, Alcator C-Mod, DIII-D, PBX-M, and TEXT tokamaks, and the Advanced Toroidal Facility (ATF) for each of FY 1995, FY 1996, and FY 1997?

Answer: The ATF has been shut down and no funding has been provided since 1994 for research on ATF. Funding levels for the other facilities follows, as requested.

Fusion Energy Sciences
(B/A in Thousands)

	Actual FY1995	Est. FY1996	Est. FY1997	
TFTR				
Fusion & Plasma Science	64,557	50,835	53,750	
Related Capital Funding	393	200	250	
Total	64,950	51,035	54,000	1/
TPX				
Fusion Technologies	39,770	0	0	
Related Capital Funding	277	0	0	
Total	40,047	0	0	
Doublet III-D				
Fusion & Plasma Science	45,225	37,595	42,000	
Related Capital Funding	4,800	700	4,000	
Total	50,025	38,295	46,000	
Alcator C-Mod				
Fusion & Plasma Science	14,799	9,945	13,000	
Related Capital Funding	1,360	0	0	
Total	16,099	9,945	13,000	
PBX-M				
Fusion & Plasma Science	5,009	0	0	
Related Capital Funding	7	0	0	
Total	5,016	0	0	
TEXT				
Fusion & Plasma Science	4,651	1,600	0	
Total	4,651	1,600	0	

Note:

1/ In FY 1997 TFTR will focus on completing the scientific objectives uniquely attainable in that device. The FEAC will review the work in progress at TFTR in mid-FY 1997 to help the Department decide as to whether to continue the planned experiments through the rest of the year into FY 1998 at the latest or whether to stop scientific operation in mid-year and use the remaining funds to prepare for decommissioning.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 11: Please describe what, if any, alternative fusion energy concepts (i.e., alternatives to tokamaks or inertial fusion driven by heavy ions) are being explored by DOE. Please provide funding levels of each of the alternative concepts, by Fusion Energy subprogram, for each of FY 1995, FY 1996, and FY 1997.

Answer: The DOE presently supports research in the alternative concepts listed below.

The Fusion Energy Advisory Committee has been asked to recommend an investment strategy for alternative concepts that will maximize the scientific information that can be gained form this research. The Committee's report will be available this summer.

Fusion Energy Sciences
(B/A in Thousands)

Concept	FY 1995	FY 1996	FY 1997
Fusion and Plasma Sciences Subprogram			
Spherical Tokamak	1750	1200	6300
Stellarator	1800	1600	2000
Reversed Field Pinch	1800	1600	2250
Field Reversed Configuration	750	750	2000
Staged Z Pinch	500	400	400
Ion Rings	550	400	400
Inertial Electrostatic Confinement	400	400	400
Spheromak/Other	400	400	600
New Initiatives			3500
Total	7950	6750	17850

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 12: What is DOE's total funding, by Fusion Energy Subprogram, in each of FY 1995, FY 1996 and FY 1997 for:

a. International Thermonuclear Experimental Reactor (ITER) activities?

Answer:

	Fusion Energy Sciences (B/A in Thousands)		
	FY 1995	FY 1996	FY 1997
ITER			
Fusion Technologies	67,554	54,927	54,930
Related Capital Funding	1,900	100	70
Total	69,454	55,027	55,000

b. ITER-related activities?

Answer:

ITER Related Activities			
Fusion Technologies	0	5,000	5,000

The results produced by the fusion science experiments and by the fusion theory and modeling activities are used by the ITER project The cost of producing these vital results is not included in the cost presented above because those results are useful to all parts of the fusion energy sciences program.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 13: What is the annual cost per FTE, by ITER Joint Work Site, for U.S. personnel assigned to one of the ITER Joint Work Sites?

Answer: The current average annual cost for a full time equivalent (FTE) U.S. person is $288 thousand at San Diego, $376 thousand at Naka, Japan, and $418 thousand at Garching, Germany. The overall average cost is $353 thousand. Many factors influence these numbers such as availability of subsidized housing and free schooling at Naka, cost of living and exchange indices, and skill and experience level of the U.S. personnel at each site.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 14: What is the current status of the ITER site selection process?

Answer: As stated in our written and oral testimony, because of the budget reductions, the U.S. can no longer consider offering to host the ITER facility here. Furthermore, because of the projection of these lower budgets into the future, any future participation of the U.S. in construction and experimentation would have to be within limited financial resources. In order to pursue the possibility of such U.S. participation, we are now considering whether to enter non-committal, pre-negotiation discussions with the other ITER Parties on possible arrangements for construction. Should the other ITER Parties decide that they can provide sufficient resources to bear the majority of the needed funding and should the U.S. decide that it is in our overall interest to join such non-committal, pre-negotiation discussions on construction arrangements, then the involved Parties could determine how to proceed to select the site for construction. In that case, it is likely that the Party or Parties providing the majority of the funding would have a determining say in which country the site would be located.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 15: When do you expect to have an agreement with the international partners about how to proceed to ITER site selection and construction commitments?

Answer: The four ITER Parties recently accepted a tentative sequence of events that could lead to decisions about construction arrangements. This acceptance represents the first level of agreement, i.e., solely the desired timing of preparatory discussions and following negotiations, with the other Parties on how we might proceed toward construction decisions. This sequence projects that the Parties should begin non-committal, pre-negotiation discussions by July of this year. This timing would maintain the possibility of reaching agreement on mutually acceptable construction arrangements before the end of the current Engineering Design Activities in July 1998. ITER Parties are deciding individually whether to enter such discussions. If the U.S. does decide to enter these non-committal discussions by July, then the further projection of the sequence is that negotiations should be initiated by the middle of 1997. Mutual readiness for negotiations would represent a higher level of agreement on how to proceed toward construction decisions, namely, substantive approaches including specific proposed responsibilities for the likely host and non-host parties.

QUESTIONS FROM REPRESENTATIVE ROHRABACHER

Fusion Energy Sciences

Question 16: What are the current estimated decontamination and decommissioning costs of the TFTR?

Answer: The decontamination and decommissioning of the Tokamak Fusion Test Reactor (TFTR) would follow the safe shutdown of the TFTR facility at the conclusion of deuterium-tritium operation. It would include dismantling and disposing of all TFTR systems that will no longer be needed. Total costs for this work, including waste disposal, were previously estimated to be about $140 million (in fiscal year 1994 dollars), assuming that the building and many ancillary systems would be reused in the future. A revised project plan for decommissioning the TFTR facility will need to be prepared in the light of current program plans and priorities. Approximately $4 million per year is required to mothball and safeguard the facility until a decision is made to begin the actual decontamination and decommissioning activity.

CITIZENS
AGAINST
GOVERNMENT
WASTE

March 25, 1996

Dr. Harlan Watson
Staff Director
Energy and Environment Subcommittee on Science
B-374 Rayburn House Office Building
Washington, DC 20515

Dear Dr. Watson:

Enclosed are the responses to the subcommittee's additional questions from the March 7 hearing on fusion research.

I just wanted to thank you for giving us the opportunity to testify on behalf of the taxpayers. Chairman Rohrbacher's commitment to fiscal responsibility is refreshing. As I have stated before, CAGW stands ready to assist you in establishing sound budgetary priorities for the Fiscal Year 1997 budget within the subcommittee's jurisdiction.

There were also two issues raised during the hearing that my be worth further investigation: private funding for fusion programs and international research. However, if the subcommittee has already examined these issues, CAGW would be most interested in what conclusions were drawn.

If CAGW can be of any further assistance to you or the subcommittee, please feel free to contact me at (202) 467-5312. Again, thank you.

Sincerely,

Scott D. English
Government Affairs Associate

enclosures

1301 Connecticut Ave., NW
Suite 400
Washington, DC 20036
202-467-5300

1. If you believe that ITER will deliver little, why should the subcommittee continue to fund it in FY 1997 and FY 1998?

As you know, the United States has made a commitment to contribute funds to the Engineering Design Activity (EDA), the first phase of ITER's development. In my testimony, I stated the importance of "international development projects that will lessen taxpayer investment on shared benefit technology," which CAGW still maintains. However, the United States' previous commitment will be fulfilled in FY 1998, giving this subcommittee ample time to determine the proper course of international fusion cooperation.

In my testimony, I also recommended that no funds should be committed towards reactor construction. CAGW believes that the tokamak reactor is not the desirable prototype to use in developing a commercially viable or successful fusion energy program. ITER will not develop a working model until 2025, and a commercially viable design will not be available until 2040. CAGW understands and supports the benefits of international cooperation, however, we question the ability of our international partners to maintain a financial commitment over such a long period, leaving the United States to shoulder the burden of a potential failure.

In my testimony, I clearly state that "...if at all possible, further international funding should be focused on basic fusion research." This will facilitate more definitive, short-term goals that can be met by an international team. Moreover, we feel that these benefits will have a more far-reaching impact on the future of fusion research than the development of questionable technology.

2. On page 7 of your prepared testimony, you recommend that FY 1997 funding for fusion should not exceed $200 million. Could you please give us specific recommendations as to how these funds might be distributed among the various fusion subprograms?

The FY 1996 fusion budget was $244 million. In my testimony, I recommended elimination of the Tokamak Fusion Test Reactor (TFTR), which was funded at $51 million in FY 1996, reductions in funding for MIT's Alcator C-MOD and General Atomics' DIII-D, which should add another $10 million in savings.

CAGW believes that reductions to the Alcator C-MOD and the DIII-D would encourage MIT and General Atomics to find private funding, which is crucial to developing a commercially viable reactor. If private companies will not commit funds in the production of commercially unattractive reactors, then why should taxpayers be expected to?

CAGW's recommendations total $61 million in savings, which would more than meet the $200 million threshold recommended in my testimony.

SECC

Safe Energy Communication Council
1717 Massachusetts Ave. NW
Suite 805
Washington, D.C. 20036
(202) 483-8491
FAX (202) 234-9194

April 5, 1996

Dana Rohrabacher
Chairman
Subcommittee on
Energy and Environment
Committee on Science
B-374 Rayburn House Office Building
Washington, DC 20515

Dear Chairman Rohrabacher:

Thank you for your letter of March 15, 1996, requesting responses to two additional questions from Members of the Subcommittee regarding my testimony, on behalf of the Safe Energy Communication Council (SECC), at the March 7, 1996 Subcommittee hearing on the Department of Energy's (DOE) Restructured Fusion Energy Sciences Program.

The first question is: **What level of funding is appropriate for fusion energy for FY 1997?** As stated in our testimony, SECC believes that the DOE should reduce its commitment to the questionable tokamak technology which consumes the majority of funds in the current fusion program. SECC recommends that funding for the Tokamak Fusion Test Reactor (TFTR) at Princeton University for FY 1997 should be restricted to termination costs and the operation of the DIII-D and Alcator facilities should only be financed with private funds. Moreover, the termination of operations at TFTR should be the highest priority for a restructured fusion energy program.

Projected funding for these three facilities for FY 1996 is: $51 million for TFTR; $38 million for the DIII-D; and $10 million for the Alcator facility, or approximately $100 million combined (See attachment). Therefore, the DOE fusion program request for FY 1997 of $256 million should be reduced to no more than $150 million which would include a reasonable allocation for termination costs for TFTR.

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Chairman Dana Rohrabacher
April 5, 1996
Page two

The second question is: **What is the appropriate mix of federal funding for renewable energy resources versus fusion energy?** In response, SECC supports the DOE FY 1997 request for energy efficiency and renewable energy which is approximately $1.1 billion and, as stated above, believes the fusion energy allocation should be no more than $150 million.

Thank you for the opportunity to provide additional information to the Subcommittee.

Sincerely,

James Adams

James Adams
Senior Analyst

Enclosure

Department of Energy
Germantown, MD 20874-1290

JAN 0 2 1996

Mr. James Adams
Safe Energy Communication Council
1717 Massachusetts Avenue, Suite 805
Washington, D.C. 20036

Subject: Request for Information about the Fusion Energy Advisory Committee Review of the Department of Energy's Fusion Energy Program and the Office of Fusion Energy's Budget

Dear Mr. Adams:

The information described below responds to your request by telephone on December 15, 1995, for information about the Fusion Energy Advisory Committee's (FEAC) review of the Department's fusion energy program and the current budget of the Office of Fusion Energy.

As requested, enclosed are copies of the presentations made to the December 7-8, 1995, FEAC meeting. Enclosure 1 is a page from the presentation made by Dr. Anne Davies, Associate Director for Fusion Energy, providing the Office of Fusion Energy's budget.

I believe this satisfies your request. If you have any further questions, we will be happy to respond.

Sincerely,

Arthur Katz

Arthur Katz, Manager
International Research
International Programs
Office of Fusion Energy
Office of Energy Research

Enclosures

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Fusion Budgets

$ Millions

	FY 1995	FY 1996 Request	FY 1996 Conference
Confinement Systems			
TPX	41	62	0
TFTR	61	40	51
DIII-D	50	52	38
Alcator C-MOD	16	17	10
Other	17	17	8
Total, Confinement Systems	185	188	107
Development and Technology			
ITER	69	82	55
Technologies	10	9	8
Materials	10	10	8
Other	3	3	4
Total, Development and Technology	92	103	75
Applied Plasma Physics	54	49	41
Inertial Fusion Energy	8	7	8
Other	18	16	13
Total Fusion Energy	***357***	***363***	***244***

Note: All figures rounded; closeout costs allocated to programs

APPENDIX III

HEARING CHARTER

The Department of Energy's Restructured Fusion Energy Sciences Program

Subcommittee on Energy and Environment

March 7, 1996
9:30 a.m.-12:00 p.m.
2318 Rayburn House Office Building

1. Purpose of Hearing

The hearing will focus on the Department of Energy's (DOE's) Fusion Energy Program, and specifically on the recent DOE Fusion Energy Advisory Committee (FEAC) report, *A Restructured Fusion Energy Sciences Program*, issued on January 27, 1996, and on the National Research Council (NRC) report, *Plasma Science: From Fundamental Research to Technological Applications*, approved in May, 1995.

2. Background

Introduction

The federal government, through the DOE and its predecessor agencies, has supported research to develop fusion energy for commercial electric power production since 1951–some 45 years. Fusion reactions, which power the sun and the stars, occur when the nuclei of lightweight atoms (such as deuterium and tritium, isotopes of hydrogen) combine together, or fuse, and release energy. Controlling fusion, which requires confining and heating deuterium and tritium nuclei to the point where they will collide and produce nuclear energy in a sustained, regulated way, presents enormous scientific and technical challenges. At such temperatures, matter exists as a plasma (an ionized state in which atoms are broken down into nuclei and electrons) that cannot be confined by any conventional container.

Most of DOE's effort has been carried out by its Office of Fusion Energy and has been devoted to the magnetic confinement approach, which uses magnetic fields to confine the plasma. A number of magnetic fusion energy (MFE) concepts have been investigated, the most advanced of which is the tokamak–a tororidal, or donut-shaped device. DOE has also devoted considerable resources to inertial confinement fusion (ICF), in which a pellet of fusion material is heated and compressed by intense laser or ion sources to such high densities that the fuel's own inertia is sufficient to contain it for the very short time for fusion to occur. Because ICF mimics, on a much smaller scale, nuclear weapon processes, DOE's Office of Defense Programs has performed much of that research for its applications to nuclear weapons physics and stockpile stewardship responsibilities.

Following is a brief and admittedly oversimplified history of DOE's Fusion Energy Program from the 1970s to present, and overviews of the FEAC report, *A Restructured Fusion Energy Sciences Program*, issued on January 27, 1996, and of the NRC report, *Plasma Science: From Fundamental Research to Technological Applications*, approved in May, 1995.

History

The 1970s saw a large increase in fusion research funding–from about $34 million in 1970 to over $350 million in 1979–as part of the overall expansion of federal energy R&D in response to the 1973 OPEC oil embargo. DOE began constructing two large MFE devices–the Tokamak Fusion Test Reactor (TFTR) at Princeton Plasma Physics Laboratory (PPPL) and the Mirror Fusion Test Facility-B (MFTF-B) at Lawrence Livermore National Laboratory–and a number of smaller MFE alternatives were pursued. In addition, the European Community and Japan also began ambitious programs, including the construction of major facilities.

The 1980s saw a retrenchment in the fusion budgets, as concern over 1970s "energy crisis" abated and the Reagan Administration sharply reduced funding for energy R&D programs, with DOE's Fusion Energy Program reaching a peak (in nominal dollars) in Fiscal Year (FY) 1984 of $468 million. By 1988, MFE Program funding had been reduced, in real dollars, to about half what it was at its 1970s peak. The MFTF-B was mothballed almost immediately after its completion in 1986 without ever operating, and research on many other alternative MFE concepts was terminated–with the result that the program became (and remains) largely concentrated on the tokamak concept and the operation of three large tokamak facilities: the TFTR at PPPL, the DIII-D at General Atomics, and the Alcator C at the Massachusetts Institute of Technology (MIT). A large tokamak successor to the TFTR, the Compact Ignition Tokamak (CIT) with the objectives of studying ignition and burning plasma physics, was proposed in the early 1980s, but was deferred in 1989.

During the 1980s, however, international collaboration in fusion expanded, particularly as a result of the initiation of the International Thermonuclear Experimental Reactor (ITER) project that began as a result of discussions between President Reagan and Soviet Leader Gorbachev at the 1985 Geneva Summit. (Currently, ITER is an international engineering design effort conducted by the U.S., the European Union, Japan, and the Russian Federation).

In 1990, then Secretary of Energy Watkins called for a high-level review of the DOE Fusion Energy Program by the DOE Fusion Policy Advisory Committee (FPAC), and the FPAC responded in September of that year by supporting a "responsible, goal-oriented fusion energy development program" aimed at achieving the goals of "at least one operating Demonstration Power Plant by 2025 and at least one operating Commercial Power Plant by 2040." FPAC estimated that such a program would require U.S. fusion program budgets (including the Defense Programs' ICF Program) to reach about $1 billion per year in constant dollars over the period 1990 to 1997 to allow the construction of new facilities–including an increase in the MFE Program from $316 million in FY 1990 to over $600 million in FY 1996 (in FY 1990 dollars). At its full budget level, FEAC called for participation in ITER Engineering Design Activities (EDA), completion of D-T experiments in the TFTR, construction of the Burning Plasma Experiment (BPX–an outgrowth of the CIT design), an increase in the base program, design of a new steady-state tokamak (which became known as the Tokamak Physics Experiment, or TPX), and increased emphases on low activation materials and nuclear technology.

Secretary Watkins adopted the FPAC findings "subject to existing budget constraints," and the Bush Administration released its National Energy Strategy in 1991 with the FPAC recommendations largely intact (i.e., an operating demonstration plant by around 2025 and an operating commercial plant by around 2040). However, Congress reduced the Fusion Energy Program's funding from $316.7 million in FY 1990 to $273.6 million in FY 1991. And, in September, 1991, Secretary Watkins followed a Secretary of Energy Advisory Board (SEAB) Task Force recommendation that the BPX project be cancelled. In FY 1992, funding rebounded to $337.21 million, but funding demands to support the TPX design and ITER activities continued to put budgetary pressure on the program.

The Energy Policy Act of 1992 (EPACT) included a mandate (Section 2114) for a five-year fusion research program "that by the year 2010 will result in a technology demonstration which verifies the practicality of commercial electric power production." EPACT also called for a broad-based program with participation in ITER activities, construction of a new major U.S. fusion machine, development of an ICF heavy-driver experiment, and increased industrial participation.

DOE first proposed a construction line item for the TPX in FY 1993, and in subsequent years. However, although Congress approved TPX design funds for FY 1993-FY 1995, it refused to approve a TPX construction line item and DOE's Fusion Energy Program budget remained relatively flat.

Reflecting continuing Congressional concern about the Program's future budgetary requirements to meet the National Energy Strategy and EPACT goals, the Conference Report (House Report 103-672) accompanying the Energy and Water Development Appropriations Act, 1995 (Public Law 103-316) urged "the President's Advisory Council on Science and Technology [sic] to undertake a review and evaluation of magnetic fusion and inertial confinement fusion energy development." The Conferees also urged the Council "to issue a report that will help shape the direction of the Nation's effort on these important energy sources for the future."

The President's Committee of Advisors on Science and Technology (PCAST) issued such a report, *The U.S. Program of Fusion Energy Research and Development*, on July 10, 1995. PCAST concluded "that DOE's program plan for continuing this effort in the decade ahead represents a reasonable approach in pursuit of the National Energy Strategy goal of operating a demonstration fusion reactor by about 2025." However, PCAST also recognized that "it is now apparent that the budgets needed to carry out the DOE's program are unlikely to be available." Consequently, PCAST "focused most of its attention on developing a budget-constrained fusion R&D strategy for the United States that could preserve the most indispensable elements of the U.S. fusion R&D effort, and the associated international collaboration, while spending about half as much over the next ten years as now planned by DOE." PCAST estimated its strategy would cost about $320 million per year and stated that a still lower budget level of about $200 million per year could not preserve key priorities.

Because of its timing, the PCAST report had little influence on the FY 1996 appropriations process. The House Appropriations Committee reported the FY 1996 Energy and Water Development Appropriations Bill on June 20, 1995, with a recommended Fusion Energy Program funding level of $229.144 million (including termination of the TPX), and the House passed the bill on July 12, 1995, by the overwhelming vote of 410 to 27. The Senate Appropriations Committee reported the bill on July 27, 1995 with an identical Fusion Energy Program funding level, and passed the bill on August 1, 1995, by voice vote.

Following appeals by the DOE during the Conference Committee's deliberations on the bill, the Energy and Water Development Appropriations Act, 1996 (Public Law 104-46) provided an additional $15 million for the Magnetic Fusion Energy Program for a total of $244.144 million, a reduction of $128.419 million, or 34.5 percent below the FY 1995 appropriation of $372.563 million.

In the Conference Report accompanying the bill, the Conferees stated that this funding was "to support a program in plasma science and fusion technology, and continue United States participation in the engineering design activities phase of the International Thermonuclear Experimental Reactor [ITER] project to which the United States is committed through fiscal year 1998."

"With little prospect for increased fusion for the fusion base program over the next several years," continued the Conferees, "it will be necessary for the program to restructure its strategy, content, and near-to-medium-term objectives. The restructured program should emphasize continued development of fusion science, increased attention to concept improvement and alternative approaches to fusion, and development and testing of the low-activation structural materials so important for fusion attractiveness as an energy source."

The Conferees directed DOE, "with the participation of the fusion community and the Fusion Energy Advisory Committee, . . . to prepare a strategic plan to implement such a restructured program, to be completed by December 31, 1995. This plan should assume a constant level of effort in the base program for the next several years; as appropriate, it should be integrated with plans of the international fusion program; and it should address the institutional makeup of a domestic program consistent with the funding assumptions."

Overview of the January 27, 1996, FEAC Report

On December 5, 1995, Dr. Martha A. Krebs, Director of DOE's Office of Energy Research, asked Dr. Robert W. Conn, Dean of the School of Engineering at the University of California at San Diego and Chair of FEAC, to respond to the Congressional directive and to consider annual Fusion Energy Program funding levels in the range of $200 million to $275 million. She also ask that FEAC consider the recommendation of the July, 1995, NRC report that plasma science should receive increased funding.

FEAC established two subcommittees to prepare the report, which was issued on January 27, 1996: (1) the Strategic Planning Subcommittee, chaired by Dr. Michael L. Knotek of Pacific Northwest National Laboratory, to analyze the policy issues; and (2) the Scientific Issues Subcommittee, chaired by Dr. James D. Callen of the University of Wisconsin at Madison, to provide scientific assessments.

FEAC endorsed the $275 million funding level, but devoted much of its attention to maintaining a budget of $250 million. At the $250 million level, FEAC recommended: (1) that TFTR operations at PPPL cease during FY 1997; (2) that there be increased operations of DIII-D at GA and Alcator C-Mod at MIT; (3) a constant level of support of the ITER EDA; (4) an increase in plasma science and alternates, with PPPL taking the lead in some of the effort, and including greater international collaboration; (5) a modest increase in materials and technology, and (6) a reduction in total DOE Program office staffing, including field offices. In the out-years at a constant $250 million level, FEAC recommended: (1) continued full operation of DIII-D and Alcator C-Mod at least through FY 2001, including some upgrades; (2) a growing portfolio of new experiments; (3) a robust theory and modeling program; (4) a fundamental plasma science budget in the range of 5 percent of the total; (5) a growing materials and technology program; (6) a potential ITER construction commitment, with any increase over the current ITER EDA level requiring budget growth; and (7) a growing set of international collaborations.

At a level below $250 million, FEAC said that there would be "a very painful conflict among implementing the restructured program, honoring our international commitment to ITER, and obtaining any further valuable scientific benefits from TFTR." At funding "significantly" below the $250 million level, FEAC said that it would be necessary to consult our international partners about reducing our ITER EDA support and possibly requiring a reduction to one major operating facility. And, at the $200 million level, FEAC said that "both the niche leadership and the resources available for the United States to put into an international collaboration as a junior partner in ITER would be only marginally attractive to the major partners. The United States would not be adequately prepared for the further development of fusion energy by the international community and would be at a significant competitive disadvantage."

At the $275 million level, FEAC recommended: (1) full operation of the TFTR, but no longer than FY 1998; (2) increased ITER EDA support; (3) more vigorous pursuit of "new directions"; and (4) more productive near-term utilization of DIII-D and Alcator C-Mod.

Overview of the July, 1995, NRC Report

In May, 1992, National Research Council (NRC) appointed members of its the Panel on Opportunities in Plasma Science and Technology and began a study of plasma science supported with funding from the National Science Foundation, the DOE, and the Office of Naval Research. The task statement to the Panel requested that this study examine virtually all aspects of plasma science and technology in the United States, assess the health of basic plasma science as a research enterprise, identify and address key issues in the field, and make recommendations to federal agencies and to the community that address these issues. Specifically, the Panel was charged with the task of conducting an assessment of plasma science that included beams, accelerators, and coherent radiation sources; single-species plasmas and atomic traps; basic plasma science in magnetic confinement and inertial fusion devices; space plasma physics; astrophysics; low-temperature plasmas; and theoretical and computational plasma science.

The Panel's report, approved in May, 1995, and made the following six recommendations:

"1. To reinvigorate basic plasma science in the most efficient and cost-effective way, emphasis should be placed on university-scale research programs.

2. To ensure the continued availability of the basic knowledge that is needed for the development of applications, the National Science Foundation should provide increased support for basic plasma science.

3. To aid the development of fusion and other energy-related programs now supported by the Department of Energy, the Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, should provide increased support for basic experimental plasma science. Such emphasis would leverage the DOE's present investment in plasma science and would strengthen investigations in other energy-related areas of plasma science and technology.

4. Approximately $15 million per year for university-scale experiments should be provided, and continued in future years, to effectively redress the current lack of support for fundamental plasma science, which is a central concern of this report. Furthermore, individual-investigator and small-group research, including theory and modeling as well as experiments, needs special help, and small amounts of funding could be life-saving. Funding for these activities should come from existing programs that depend on plasma science. A reassessment of the relative allocation of funds between larger, focused research programs and individual-investigator and small-group activities should be undertaken.

5. The agencies supporting plasma science should cooperate to coordinate plasma science policy and funding.

6. Members of the plasma community in industry and academe should work aggressively for tenure-track recognition of plasma science as an academic discipline, and work with university faculty and administrators to provide courses in basic plasma science at the senior undergraduate level."

3. Issues and Questions

As noted earlier, the hearing will focus on the Department of Energy's (DOE's) Fusion Energy Program, and specifically on the recent DOE Fusion Energy Advisory Committee (FEAC) report, *A Restructured Fusion Energy Sciences Program*, issued on January 27, 1996, and on the National Research Council (NRC) report, *Plasma Science: From Fundamental Research to Technological Applications*, approved in May, 1995.

Among the issues and questions of interest are the following:

- What is the potential role of the Fusion Energy Program in meeting long-term energy needs, and what level of research is justified by that role?

- What is the potential role of the Fusion Energy Program in meeting long-term non-energy needs, and what level of research is justified by that role?

- To what extent is the FEAC report responsive to the Congressional directives?

- To what extent is the FEAC report responsive to the NRC report?

- Can cost-sharing through international collaboration be pursued more effectively, and if so, how?

- What are the prospects that a tokamak fusion reactor will be an economically-viable and environmentally-acceptable commercial electric power producer, and it what time frame?

- What are the perceived advantages and disadvantages of a conventional tokamak reactor?

- What is the role of ITER, and why does continued U.S. participation in ITER make sense?

- What is the rationale to pursuing alternate concepts as part of a fusion program?

- What is the current status of knowledge for alternate concepts?

- What fraction of the fusion budget should be invested in the exploration of alternate and fundamentally different approaches to fusion energy that, at a minimum have the potential of leading to a viable commercial fusion reactor product?

- Which fusion plasma physics questions can only be addressed by large tokamak facilities, and which can be addressed by smaller long-term research programs dedicated to isolating and studying fundamental plasma phenomena in a more complete and systematic manner?

- Why is it necessary to maintain operation of three large tokamak facilities?

- Have there been advances in computational techniques that could lead to less reliance on large experimental devices, and if so, are they exploited adequately?

Witnesses will include representatives of the DOE's Fusion Energy Advisory Committee, the National Research Council's Panel on Opportunities in Plasma Science and Technology, who will present the findings, conclusions and recommendations of the FEAC report, *A Restructured Fusion Energy Sciences Program* and the NRC report, *Plasma Science: From Fundamental Research to Technological Applications*. Also testifying will be members of the fusion community not represented on the FEAC or the NRC Panel, and public interest groups.

A Restructured Fusion Energy Sciences Program

Advisory Report
Submitted to
Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy

By
The Fusion Energy Advisory Committee

January 27, 1996

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January 27, 1996

Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy
Washington, DC 20585

Dear Dr. Krebs:

The Fusion Energy Advisory Committee (FEAC) has proceeded to address the charge in your letter to us of December 5, 1995 with the conviction that the United States must field a program that seizes the opportunities of today, in a restructured format, to promote progress in fusion science and technology. This is a time of tremendous progress and opportunity in fusion. Yet, despite significant scientific and technical progress, constrained budget prospects place the United States fusion program at a dramatic crossroads.

In response to your charge letter, we are pleased to transmit the enclosed report, "A Restructured Fusion Energy Sciences Program," hereafter referred to as the Report. This Report was prepared to provide recommendations on how to restructure the fusion program in the light of congressional guidance and budgetary realities. Your letter to the FEAC referred to the Conference Report accompanying the FY 1996 appropriations bill, which indicated the necessity of restructuring the fusion program's strategy, content and near- to medium-term objectives, assuming a constant level of effort in the base program. You asked for advice on the strategy for the fusion program and plan for implementation of that strategy, including institutional considerations and the role of the International Thermonuclear Experimental Reactor (ITER) and other international collaborations in the program. You also asked the FEAC to consider the broader issue of plasma science that underpins fusion energy and that has important applications in science, technology, and industry.

To establish a mission, a set of goals, a strategy, and an implementation plan, the FEAC created in December two subcommittees: the Strategic Planning Subcommittee, chaired by Dr. Michael L. Knotek, to analyze the policy issues, and the Scientific Issues Subcommittee, chaired by Dr. James D. Callen, to provide scientific assessments to inform the deliberations. The two subcommittees worked closely together and prepared the Report we are transmitting to you. As part of the process, views were solicited from the entire fusion community, and key laboratories and facilities were visited by subcommittee members during the review. The FEAC wishes to thank officially the members of both subcommittees for their work, as well as those who presented material to the subcommittees, including our international partners and the many people who forwarded viewpoints to the subcommittee.

The FEAC voted unanimously to accept the Report "A Restructured Fusion Energy Sciences Program" and unanimously endorses the mission and policy goals given in the Report. By a vote of 10 to 2, the FEAC also endorses the findings and recommendations contained in the body of the Report.[1] The recommended restructured program is consistent with the new mission and policy goals and fits within the upper range of annual budget guidelines that you provided in your December 5th, 1995 charge to the FEAC.

In summary, the Report recommends that the mission of the U.S. Fusion Energy Sciences Program be modified to be consistent with both the most recent programmatic guidance and the level of resources provided by Congress. The new mission is to advance plasma science, fusion science and fusion technology--which constitute the knowledge base needed for an economically and environmentally attractive fusion energy source. The FEAC recommends, in no priority order, three policy goals: advance plasma science in pursuit of national science and technology goals; develop fusion science, technology and plasma containment innovations as the central theme of the domestic program; and pursue fusion energy science and technology as a partner in the international effort.

In 1995, the President's Committee of Advisors on Science and Technology (PCAST) reviewed the U.S. magnetic fusion program. In response to their charge, they recommended a $320M figure as a minimum annual funding level for a viable fusion energy program. This funding level would have allowed the United States to maintain a leadership role in the world effort to develop fusion power. The subsequent decision to fund the program below this minimum level and the guidance to expect flat out-year budgets have completely changed the position of the U.S. magnetic fusion effort relative to Europe and Japan. Efforts to build a next-generation world class experiment in the United States were abandoned, U.S. participation in the international burning plasma program on ITER was reduced, and many other important U.S. fusion science activities were curtailed.

The historically strong United States leadership role in the world magnetic fusion energy program came to an end with the decision on FY 1996 funding. However, we conclude that the United States can still play an important supporting role in magnetic fusion energy development, but only by recognizing the new dependence of U.S. efforts on the activities and decisions of Europe, Japan, and the Russian Federation. As such, progress will depend on maintaining a balance of domestic and international activities.

As requested by the Department in your charge letter to us of December 5, 1995, the FEAC and its subcommittees considered annual program funding levels in the range of $200 million to $275 million. The subcommittee examined as its base case a constant level-of-effort budget of $250 million. The restructured Fusion Energy Sciences Program in this case is described in great detail in the Report. This restructured program is consistent with the new mission statement and is built around the three policy goals.

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[1] It is inevitable that, given the tight deadlines imposed by the budget process, small inconsistencies may appear between the body of the Report and some of the appendices. The FEAC believes that these inconsistencies are not consequential and emphasizes that our strong endorsement is of the findings and recommendations in the body of the Report.

At this budget level, restructuring begins by providing incremental funding to pursue basic plasma science, to pursue plasma-containment research (plasma science and alternative concepts), and to achieve greater utilization of DIII-D and C-Mod. These priorities require, however, that Tokamak Fusion Test Reactor (TFTR) at the Princeton Plasma Physics Laboratory (PPPL) cease operation during FY 1997, foregoing the remaining unique scientific output possible from that facility.[2]

At the lower funding levels, (below $250 million per year), it is not possible to implement the goals of the restructured program, which include honoring our international commitments to the ITER engineering design activity (EDA) and obtaining further valuable scientific benefits from our existing experimental facilities. The FEAC does not recommend these lower levels of funding, At the highest budget level considered ($275 million per year), the restructuring would proceed with greater effectiveness (e.g., exploiting high priority scientific results before shutting down a major facility; strengthening our support for the international commitment of the United States to the ITER EDA; and allowing more vigorous pursuit of the new directions that are at the core of the restructuring), and we recommend this case to the Department.

With respect to international cooperation and the ITER effort, the broad physics and engineering challenges that ITER addresses are largely generic to any next step toward the goal of fusion energy. Therefore, the science and technology research within both the ITER EDA and the U.S. core program that addresses ITER's challenges is appropriate and valuable. Such work is also consistent with the recommended fusion energy science mission for the program. The FEAC finds that the most cost-effective way for the United States to maintain a strong research effort in burning-plasma physics is through continued participation in the ITER EDA and the ITER process. Further, the ITER EDA will provide a robust and thorough engineering design based on extensive R&D activities. This design has already highlighted certain important physics issues and has become a driver in current experimental and theoretical programs. It is important to recognize that the ITER EDA is the single most important mechanism for American industry to participate in fusion development.

The Report contains several recommendations regarding program governance. We strongly encourage efforts to enhance opportunities for "grass roots" participation in the scientific and programmatic leadership of the Fusion Energy Sciences Program. Such participation was mobilized effectively to develop the restructured plan contained in our Report. Continued and enhanced leadership from the "grass roots" will promote community consensus; it will sharpen the focus on the mission and goals; it will help foster a climate conducive to innovation; and it will strengthen outreach to the stakeholders, related science fields, and the public. One mechanism for this leadership participation is the continuation of the Science Subcommittee of the FEAC.

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[2] The FEAC did not assess or include closeout costs associated with the termination of facilities and programs. If these costs must be funded by the Fusion Energy Sciences Program, resources to do so must be added to the budget to accomplish the described restructured program.

Finally, as the nation's program-dedicated laboratory for fusion science, the PPPL must provide the leadership necessary for the restructured national Fusion Energy Sciences Program to succeed. The PPPL provided such national leadership for the Tokamak Physics Experiment (TPX) project, and we want to emphasize the importance of maintaining this critical resource and capability.

We are confident that the recommendations contained in the Report are responsive to the concerns raised by Congress and will allow the DOE Fusion Energy Sciences Program to advance the scientific knowledge-base needed for an economically and environmentally attractive fusion energy source for the nation and the world.

Sincerely,

Bob Conn

Robert W. Conn, Chair
Fusion Energy Advisory Committee,
on behalf of the Fusion Energy
Advisory Committee and its two
Subcommittees

Enclosure

A Restructured Fusion Energy Sciences Program

Advisory Report
Submitted to
Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy

By
The Fusion Energy Advisory Committee

January 27, 1996

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Executive Summary

In response to a request from the Director of the Office of Energy Research, this report provides recommendations from the Fusion Energy Advisory Committee (FEAC) on how to restructure the fusion program in light of congressional guidance and budget realities. The restructuring is based on: a survey of the field, including science and technology issues, capabilities, and programs; a new mission for the U.S. Fusion Energy Science program; and a set of policy and science goals. The report includes: the principles and outline of the restructured program; an analysis of the impact of annual budgets ranging from $200M to $275M; and recommended actions to implement the transition and establish a governance system for the restructured program. In this funding range, the United States must concede leadership of the world's fusion energy development effort to Europe and Japan.

The underlying theme of the restructuring is to redirect the program away from the expensive development path to a fusion power plant to focus on the less costly critical basic science and technology foundations. The proposed new mission and supporting policy goals are as follows:

MISSION:* *Advance plasma science, fusion science, and fusion technology — the knowledge base needed for an economically and environmentally attractive fusion energy source.

POLICY GOALS:

- ***Advance plasma science in pursuit of national science and technology goals.***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program.***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

As a first step, we recommend the adoption of the mission and goals and renaming the program the **Fusion Energy Sciences Program**, to reflect accurately the new focus. By incorporating the new mission and goals, the restructured program can fit within a constant annual budget and does not require increased outlays for construction of new facilities.

During the subcommittee process, we identified and assessed eight scientific and programmatic issues involved in the transition to the restructured U.S. fusion program: (1) Fusion Program Scientific Goals, (2) Development of Basic Plasma Science[1], (3) Theory and Computation, (4) Major Tokamak Facilities, (5) Plasma Confinement Research (Alternative Concepts) (6) Inertial Fusion Energy, (7) International Thermonuclear Experimental Reactor (ITER), and (8) Fusion Materials and Technology. Summary assessments are included in the report.

Budget Impacts

The FY96 budget of $244M (a 32% reduction compared to FY95) forced hard choices and has had serious consequences. Looking toward the future, all the funding scenarios require us to close scientifically productive domestic facilities for budgetary, not scientific, reasons, in order to achieve cost-effective utilization of the remaining facilities and to begin the pursuit of new opportunities and directions essential to the restructuring.

The funding level in FY97 is critical, and it is possible only with $275M to move forward briskly on restructuring while accomplishing the full programmatic scope directed in the FY96 Appropriations Report from Congress. Below $250M, it would be necessary to consult again with our international partners on an affordable U.S. share in the ITER Engineering Design Activities (EDA). The restructuring transition would be prolonged and complicated and result in a program that is marginalized in the international context.

In FY98 and beyond, stable funding at or above the FY96 level of effort would allow the United States to remain abreast of international development across fusion science and technology and to continue world leadership in selected specialties. Such niche leadership is essential for us to be sought by international partners as a valued participant, though perhaps minor monetary contributor, for internationally launched major facilities, defining the path to fusion energy production. At all budget levels, an increase in international cooperation (creation of flexible mechanisms to exploit the capabilities of international facilities jointly) is of paramount importance.

Governance

Critical to the success of the restructured program is immediately starting a governance transition, as a mechanism for guiding and implementing the major programmatic changes in a smooth and effective manner.

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[1] Leadership of the Nation's plasma science research effort is recommended in partial response to the recent Report of the Panel on Opportunities in Plasma Science and Technologies, *Plasma Science from Fundamental Research to Technological Applications*, National Academy Press, Washington, D.C., 1995.

The **Fusion Energy Sciences Advisory Committee** to DOE's Office of Energy Research, assisted by a continuing **Science Subcommittee**, should advise ER-1 and the program office on policy, goals, priorities, budget, direction, program balance, and governance.

Fusion Energy Sciences Program Management must be reorganized and downsized to match the science-dominated mission, and rely significantly on peer review as the primary input for funding allocations.

Specific programmatic reviews should be conducted and integrated during the remainder of FY96 to help set the technical priorities of the restructured program, given a funding level not to exceed the FY97 President's Budget Request.

- **A Major Facilities Review** in association with a **User Access Working Group**
- **An Alternative Concepts Review**
- Planning for **review of the ITER EDA and its results** and to establish **criteria for a decision** on future U.S. participation.

The current federal budget realities and the lack of a perceived domestic energy shortage demand program restructuring in accordance with the recommendations in this report, so that the U.S. program will focus on the science and technology foundations for a future or internationally led push toward fusion energy. United States involvement in fusion research and development will continue to be "a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research - - plasma physics - - that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and industry."[2] Enhanced public outreach is needed to keep the public, stakeholder groups, and the broader scientific community fully informed.

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[2] *The U.S. Program of Fusion Energy Research and Development*, Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995.

A Restructured Fusion Energy Sciences Program

1 Introduction

1.1 Background: In the Conference Report for the FY 1996 Energy and Water Development Bill, which included a significant reduction in the funding to pursue fusion energy, the Conferees directed the fusion energy program "to restructure its strategy, content, and near- to mid-term objectives." The Conferees further stated that "the restructured program should emphasize continued development of fusion science, increased attention to concept improvement and alternative approaches to fusion, and development and testing of low-activation structural materials so important to fusion's attractiveness as an energy source." While there exists an acknowledged long-term need for new energy sources for this nation and the world, the lack of a near-term need and the current national goal of balancing the budget necessitates that we redirect the fusion energy program to more closely match national needs over the near to mid-term, while positioning ourselves properly for the future.

The Conferees instructed the Department of Energy, with the participation of the fusion community and the Fusion Energy Advisory Committee (FEAC), to prepare a strategic plan to implement a restructured fusion program, assuming a constant level of effort in the base program over the next several years. On December 5, 1995, the Director of the Office of Energy Research, Department of Energy, requested FEAC to provide recommendations on how to restructure the fusion program in light of congressional guidance and budget realities (see Attachment 1). This report responds to that request.

1.2 Process Followed: To establish goals, a strategy, and an implementation plan, FEAC established two subcommittees: The Strategic Planning Subcommittee (SPS), specifically charged with developing the deliverables, and the Scientific Issues Subcommittee (SciCom), charged with providing scientific assessments to inform the deliberations. The two subcommittees worked closely throughout the study on the analysis of the key issues. In addition, extensive efforts were made to solicit (including through the Internet) opinions, proposals, facts and positions from the fusion community and interested stakeholders. Over 200 communications were received, and they generated considerable debate. Actions included the following:

- Three open meetings were held, at the Princeton Plasma Physics Laboratory (PPPL), General Atomics in San Diego, and the San Diego International Thermonuclear Experimental Reactor (ITER) Cocenter.

- The chairman of the SPS visited the facilities and held discussions at the Massachusetts Institute of Technology (MIT) Plasma Fusion Center.

- The subcommittees held discussions with the heads of the Japanese, European Union, and Russian fusion efforts, and the head of the ITER design team.

- Several discussions were held with DOE officials and staff, officials from the Office of Science and Technology Policy (OSTP) and the Office of Management and Budget (OMB), and the staff of relevant congressional committees.

The subcommittees: specifically carried out a survey of the field, including science and technology issues, capabilities, and programs; defined a new mission for the U.S. Fusion Energy Science program; established a set of policy and science goals; developed the principles and outline of a restructured program; studied the impact of budgets on goals; defined a program that assumes a constant level of effort of $250M (FY97 dollars) (as called for in the Conference Report Language) and describes the impacts of budgets up to $275M and down to $200M; and developed principles of governance to guide the transition of the program to the new structure.

1.3 Assessment of Fusion Energy Research: Our assessment of fusion energy research focused on identifying key science and technology issues affecting fusion policy and on evaluating the capabilities and strategies of fusion programs throughout the world. The recently completed report from the Fusion Review Panel of the President's Committee of Advisors on Science and Technology (PCAST),[1] the report from the Panel on Opportunities in Plasma Science and Technology published by the National Research Council (NRC),[2] and the Draft Strategy for a Restructured U.S. Fusion Energy Research Program prepared three months ago by DOE's Office of Fusion Energy (OFE) using input from leaders of the U.S. fusion program were carefully evaluated as part of our assessment. Overall, our subcommittee strongly endorsed the conclusion from the PCAST report describing U.S. funding for fusion research and development (R&D) as "a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research — plasma physics — that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and in industry."

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[1] *The U.S. Program of Fusion Energy Research and Development*, Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995.

[2] *Plasma Science from Fundamental Research to Technological Applications*, Report of the Panel on Opportunities in Plasma Science and Technology, National Academy Press, Washington, DC, 1995.

During our subcommittee process, we also identified scientific and programmatic issues within the present U.S. fusion program. For each of these areas, we prepared an assessment that helped to motivate and define our Restructured Fusion Energy Sciences Program. These issue summaries also illustrate how the implementation of our new policy goals can strengthen key research areas and set necessary budgetary priorities. These issue summaries are attached to the end of this report, and they serve to expand and clarify program needs and solutions concerning (1) Fusion Program Scientific Goals, (2) Development of Basic Plasma Science, (3) Theory and Computation, (4) Major Tokamak Facilities, (5) Plasma Confinement Research (Alternative Concepts) (6) Inertial Fusion Energy, (7) International Thermonuclear Experimental Reactor, and (8) Fusion Materials and Technology.

2 A New Mission

We recommend that the mission of the U.S. Fusion Energy Sciences Program be modified to be in accord with both the most recent guidance and resources made available by Congress. Specifically, the fusion energy program outlined in the Energy Policy Act of 1992 (e.g., a technology demonstration by 2010 that would verify the practicability of commercial electric power production) cannot be realized at the budget levels now foreseen. Consequently, we recommend that the mission for the U.S. program be restated to put it more in a world context, reflecting the reality that the leadership of the world's fusion energy development effort now lies outside the United States, as well as emphasizing both its science and technology goals. Based on discussions with our international partners, and guidance we have received, this mission can best be stated as:

MISSION: Advance plasma science, fusion science, and fusion technology – the knowledge base needed for an economically and environmentally attractive fusion energy source.

This mission and intent can best be met with the following set of policy goals:

- ***Advance plasma science in pursuit of national science and technology goals;***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program; and***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

These goals have the same major elements as the PCAST key priorities, but emphasize more the science foundation of the program. They are elaborated below.

By incorporating the new mission and goals, the restructured program can fit within a constant annual budget and does not require increased outlays for construction of new facilities.

2.1 Advance Plasma Science: Plasma science is a cornerstone of the scientific infrastructure of the country, and is a prerequisite competency to pursue many national science and technology goals, from national security to industrial processing and astrophysics, requiring major advances at the frontiers of science and numerous enabling technologies. Fusion Energy is the Grand Challenge of Plasma Science, and is "...the largest driver for the intellectual development of Plasma Science." The people tackling the scientific and technological issues involved have created a wellspring of knowledge and capability which is a national asset of enduring value. As the centerpiece of the nation's plasma science infrastructure, the restructured Fusion Energy Sciences Program must explicitly move to broaden its intellectual and institutional base in fundamental plasma science and attendant enabling technologies, preferably in partnership with other agencies.

2.2 Develop Fusion Science and Concept Innovation: Fusion science has seen major advancements over the past 30 years, with attendant benefits that strengthen the national science and technology base. Fusion Science is the combination of plasma science, related disciplines, and enabling technologies required to develop fusion as an energy source. The restructured program will focus on continued development of fusion science with increased attention to concept innovation and alternative approaches to fusion. Key enabling technologies, including radiation resistant, low-activation material and blanket technology, central to fusion's environmental and economic attractiveness, must be pursued. Increased international collaboration must be a mechanism for maximum benefit from the world capital investment in advanced facilities. U.S. strengths in theory and modelling, diagnostics, and other areas where we can provide unique resources should be increasingly brought to bear in partnership on all domestic and international facilities to achieve critical scientific and technological goals with maximum dispatch and minimum cost. The restructured program will also explicitly take the lead in reaching out to other disciplines and areas of national need for mutual benefit.

2.3 Pursue Fusion Energy as an International Collaboration: Fusion energy holds the potential to provide a vital, environmentally attractive energy option for a growing world population in the next century and beyond. The pursuit of fusion energy is of such cost and complexity that it can only be achieved through international collaboration.

As discussed in the PCAST report, much of the world will find energy availability a critical roadblock to progress in the next century and beyond, and it could become a major determinant of global political stability, as it has in the past. Among the major supporters of fusion energy development, the United States has larger domestic fossil energy resources and potential for renewable energy than the European Union and Japan. As a result, fusion energy research has a relatively lower priority in the United States. Europe and Japan are supporting a much larger fusion research effort than the United States to meet their own future needs, to provide for international energy options and stability, and to support trade with the developing world.

While the development of a new energy source is not a critical near-term need in this country, it is in our national interest to be a credible partner in this international pursuit and to pursue long-term energy options that alleviate the environmental problems of fossil fuels. National benefits include providing energy security for a growing world population, preventing our own scientific and technological isolation, positioning ourselves as a world provider of energy technology, and meeting our commitments as a reliable partner. To be a strong partner in this long-range quest, we need a vigorous domestic program in fusion science and technology. The domestic program provides the basis for leadership internationally and positions our industries to field and exploit the technology when it is mature.

The international collaboration is now focussed on the scientific base, technology development, and engineering design necessary to construct a long-pulse burning plasma experiment — the ITER. The restructured program should make every effort to remain a strong partner in the worldwide fusion program and provide a structure for a coordinated international effort. In particular, the restructured program must strive to meet our commitment to the successful completion of the ITER Engineering Design Activities (EDA), to leave open the possibility of U.S. participation in ITER construction and/or other international collaborations to advance fusion science and technology toward electricity generation, at a moderate-cost, but high-leverage investment for the United States.

3 Implementing Principles

In executing the mission of the new U.S. Fusion Energy Sciences Program, the committee recommends that ten principles be applied:

1. ***Science Focus.*** Fusion science represents a combination of interrelated disciplines that advance through large- and small-scale experimentation, theoretical and computational modelling, and materials and technological innovation. We envision the restructured fusion program to be integrated around a set of national and international experimental and theoretical resources and interaction with numerous scientific communities.

2. ***Energy Goal.*** The new science program serves the U.S. DOE's energy mission. That is, the program supports science with the long term purpose of enabling the development of an abundant, safe, environmentally attractive, and cost-competitive energy source.

3. ***Reliability as an International Partner.*** Consultation with our international partners should be a major ingredient of the evolution of our commitment to the ITER EDA.

4. ***Complementarity to the International Effort.*** The program should be designed in such a way that it complements the international effort to field a fusion energy source in the first half of the next century. This principle positions the United States to reenter an international effort quickly, whenever it becomes nationally advantageous to do so.

5. ***Leadership in Selected Areas.*** Areas of U.S. expertise having high leverage in the international effort to develop fusion energy should be identified and pursued vigorously with healthy funding. A few examples of these areas are plasma theory and computation, high-performance operating modes for tokamaks, low-activation materials, diagnostics, and plasma confinement innovations.

6. ***Scientific Excellence.*** All elements of the fusion program should be peer reviewed and held to the highest standards of scientific excellence. This principle is particularly important to guide program restructuring.

7. ***Facility Balance.*** An appropriate balance should exist between a few well-integrated, large national facilities investigating a spectrum of fusion science issues, and smaller facilities more narrowly focussed on well-posed scientific investigations. Our larger facilities produce plasmas with conditions resembling those found in future fusion energy sources. They provide scientists with unprecedented opportunities to explore fusion and plasma science, and they should be used as centers of collaboration both nationally and internationally. Smaller facilities can be constructed at significantly reduced investment; thus, they provide an effective route to high-risk, high-benefit experimentation.

8 ***Importance of a National Laboratory for Plasma Physics.*** The Princeton Plasma Physics Laboratory (PPPL), the nation's program-dedicated laboratory for fusion science, is a critical national resource for the fusion program. As an internationally recognized national center of excellence, it must maintain a critical mass of core competencies for national leadership and international collaboration for fusion science. Its technical infrastructure represents decades of investment and must be effectively utilized.

9. ***Education and Human Resources.*** A strong educational component is essential to ensure the lasting benefits resulting from the fusion program, to attract the brightest people to address the challenges facing fusion, and to maximize the application of our expertise and knowledge base to related fields such as photonics, surface science, semiconductor fabrication, and coherent radiation sources.

10. ***Diversity of Participation.*** Project participants should represent a geographically, scientifically, and institutionally diverse set of intellectual resources.

4 Budget Impacts

In its budget deliberations, FEAC and its subcommittees focussed on program funding levels in the range of $200M to $275M. In all the cases, most especially below $250M, facilities are terminated, and closeout and decommissioning costs will be required. FEAC is unable to assess these costs, and recommends that the DOE provide for them outside the scope of the program, since their inclusion in the program budget would significantly erode the productivity of the program and seriously compromise the restructuring.

The FY96 budget of $244M (a 32% reduction compared to FY95) has had a number of consequences: a major loss of scientific and technical manpower; the foregoing of further significant capital facilities; termination of some critical enabling technologies; a curtailment of university research programs in experimental plasma physics; a subcritical and inefficient utilization of our major tokamak and other facilities; reductions in numerous programs in industry and the national laboratories; and a renegotiated, minimal contribution to the ITER EDA. These were hard choices among meeting our international commitments to the ITER EDA, our utilization of world-class facilities located within the United States (facilities which are in a period of unprecedented scientific productivity), and terminating valuable elements of the core U.S. scientific program.

Restructuring in the absence of a thoughtful transition could further lose significant useful human and capital assets that are at the heart of our current strong position in this field. We recognize the need to close existing domestic facilities to allow pursuit of new opportunities and directions that keep the field vital, exciting, and productive in this depressed budget climate. As we restructure the fusion science program with a new set of policy goals and priorities, and constant budgets, a vital and viable long-term program must be developed that creates scientific progress for the nation and a real contribution to our goals.

The Conference Report Language states:

> "The high cost of fusion development points to the increasing importance of international cooperation as a means of designing, building, and financing major magnetic fusion facilities in the future. Because the United States has committed to such an approach, it is crucial that a restructuring of the fusion program maintain a strong domestic base and not undermine our credibility as a reliable international partner."

An important factor in our deliberations is that at a relatively constant level of effort, a strong domestic program and international collaborations are constructively complementary goals, while at the lower budget levels we were asked to consider they become conflicting and divisive, especially during the transition phase over the next two years. The great challenge is to find the proper balance among the program elements, in both the near and longer terms.

4.1 The Constant Level of Effort ($250M) Case: In response to the congressional budget guidance of a constant level of effort, we will speak to a $250M FY97 budget. To move resolutely to a restructured program, the following must occur in FY97:

- ***TFTR operations must cease during FY97, running at high utilization for part of the year, and at a significant reduction from a full utilization budget.***

  With this action, we have made some painful choices. The TFTR is a $1B facility that is now in a period of extraordinary scientific productivity, exploring newly discovered regimes with new diagnostics in a deuterium-tritium environment. For lack of ~$25M, we are forced to terminate this program prematurely, foregoing unique scientific opportunities to study plasma self-heating and reacting-plasma phenomena. It is unclear when these lost opportunities would return.

- ***DIII-D and C-Mod and the leading smaller facilities must move toward full, maximally productive utilization.***

- ***The ITER EDA commitment is constant in as-spent dollars at the renegotiated lower level, with scope determined in consultation with our international partners.***

- ***There must be increases in plasma science and alternates, with PPPL taking the lead in some of the effort, and including greater international collaboration.***

- ***There must be modest increases in materials and technology budgets.***

- ***There must be a reduction in the total DOE program staffing, including field offices.***

This plan begins the recommended redirection on a flat budget. It better utilizes the surviving facilities, which are currently subcritical. It continues to strain our ability to deliver our ITER commitment for the remainder of the ITER EDA. It abandons the unique scientific opportunities lost by the premature termination of TFTR.

In the outyears in the constant level of effort case, we envision the following:

- ***Continued full utilization of DIII-D and C-Mod at least through 2001, including some upgrades, as user facilities to pursue the rich science to be gained.***

- ***A growing portfolio of new experiments including one or two smaller but scientifically aggressive new facilities, at least one taking advantage of the PPPL infrastructure.***

- ***A robust theory and modelling program.***

- ***A fundamental plasma sciences budget in the range of 5% of the funding for the base program.***

- ***A healthy materials and technology effort, including capturing that part of our technology program now dedicated to the ITER EDA, and redirecting it to new technologies.***

- ***A potential commitment to ITER construction determined by a rigorous review of the ITER design and in consultation with our international partners, but with any increase over the current ITER EDA level requiring overall budget growth.***

- ***A growing set of international collaborations which focus our niche strengths on major science and technology goals.***

4.2 At Lower Funding Levels: There is a very painful conflict among implementing the goals of the restructured program, honoring our international commitment to ITER, and obtaining any further valuable scientific benefits from TFTR. The TFTR's premature termination would lose imminent discoveries central to the advancement of fusion science and put them out of reach for many years.

Significantly below the $250M level, while we envision a continued level of support for the ITER EDA, it would be necessary to consult again with our international partners on the level and nature of our participation in the final two years of the fully integrated ITER design and technology-development phase. Further withdrawal from the ITER EDA would severely strain the relationship with our partners, complicating any attempts to strengthen science and technology collaborations with them in other areas.

The productivity of all major U.S. facilities would be adversely impacted, possibly requiring a reduction to only one major operating facility. Opportunities for new scientific initiatives would be severely constrained, defeating the key objectives of the program restructuring. The nation's technical credibility as an international collaborator would be further damaged by the shrinking of the domestic base. At the $200M level both the niche leadership and the resources available for the United States to put into an international collaboration as a junior partner in ITER would be only marginally attractive to the major partners. The United States would not be adequately prepared for the further development of fusion energy by the international community and would be at a significant competitive disadvantage.

4.3 The $275M Case: A $275M budget in FY97 would allow the restructuring to proceed with much less destructive consequences than the $250M case. Specifically,

- It would allow the highest-priority scientific opportunities on TFTR to be exploited before terminating its operation during FY98.
- It would enable us to strengthen our support of the ITER EDA and restore some of our original commitments.
- It would allow more vigorous pursuit of new directions that are at the core of the restructuring.
- It would allow more productive near-term utilization of DIII-D and Alcator C-Mod.

For these reasons, we conclude that the goals of the restructured fusion program can be accomplished most effectively at a funding level of $275M.

In FY98 and beyond, stable funding at $275M would allow the United States to pursue some aggressive small-scale fusion science initiatives after the TFTR is closed, to remain abreast of international developments in fusion science and technology, and to continue world leadership in selected specialties. Such niche leadership is essential for us to be sought by international partners as a valued participant, though perhaps minor monetary contributor, for internationally launched major facilities, starting with ITER, defining the path to fusion energy production.

5 Governance

5.1 Purpose and Principles: The governance system for the restructured Fusion Energy Sciences Program needs to: ensure focus on the policy and scientific goals; provide oversight; establish an open process for obtaining scientific input for major decisions, such as planning, funding, and terminating facilities, projects, and research efforts; build community consensus; orchestrate international collaboration fully integrated with the domestic program; and promote effective outreach to and communication with related scientific and technical communities domestically and internationally, industrial and government stakeholders, and the public.

General governance principles to accomplish these goals include open communication within the fusion community and with stakeholders, managing transitions to be constructive, not destructive, "due process" for major decisions, no entitlements, and community consensus on priorities and balance consistent with the government's agenda. Critical to the success of the restructured program is immediately starting the governance transition, as a mechanism for guiding and implementing the major programmatic changes inherent in the restructuring in a smooth and effective manner. Some elements of the recommendations below are already in place and can serve as a foundation for restructured governance.

5.2 Fusion Energy Sciences Advisory Committee to the Office of Energy Research: This committee, populated by stakeholders, practitioners, and scientific 'outsiders,' should advise ER-1 and the program office on policy, goals, priorities, budget, direction, and program balance. It can be used by the DOE to obtain community and stakeholder input on a broad spectrum of scientific and policy issues as they arise (or in anticipation thereof). An immediate priority is to oversee and provide policy integration for the specific immediate actions recommended here. It should recommend an appropriate governance system for the program for the longer term. The membership must be reconfigured to oversee and institutionalize the changes involved in restructuring.

A continuing Science Subcommittee, including experts representing the diverse fields of science and engineering underpinning fusion science as well as selected other fields should be established to provide an important channel of communication from the full breadth of the fusion community to the Fusion Energy Sciences Advisory Committee, and to provide the best possible scientific input for priority setting. Beyond providing input to the Fusion Energy Sciences Advisory Committee, the Science Subcommittee would be a new locus of scientific leadership and offer a mechanism for the fusion community to build consensus.

5.3 DOE Fusion Energy Sciences Program Management: The primary role of program management is the funding, management, administration, and oversight of the program. To accomplish this role effectively, it is essential to preserve the core of highly qualified, scientifically knowledgeable staff, but to reorganize and downsize (headquarters and field elements) to match the science-dominated mission, now replacing the milestone-driven energy development mission. The program structure and budget categories should become aligned with the goals of the restructured program. A peer review process should be used as the primary mechanism for evaluating proposals, for assessing progress and quality of work, and for initiating and terminating facilities, projects, research programs, and groups. This approach will allow program management costs to be brought into parity with the other program offices in the Office of Energy Research.

5.4 Specific Immediate Actions: The remainder of FY 1996 presents a limited window for devising and implementing suitable processes and for starting to align the program for a smooth transition into FY97. Without prejudging the optimum complement of responsive, flexible, and minimally bureaucratic processes needed for governance of the restructured program in the long term, FEAC recommends the following specific immediate actions:

- A major facilities review should be held as soon as practicable, to examine and evaluate the progress, priorities, and potential near-term contributions of TFTR, DIII-D, and C-Mod (other facilities should be included if appropriate) in order to develop an optimum plan for gaining maximum scientific benefit from their operation, at a funding level not exceeding the FY 1997 President's Budget Request for fusion.

- A User Access Working Group, composed of facility managers and user representatives, should be convened to work with the Program Office and facilities to develop and publicize a mechanism for encouraging, enabling, and funding the highest quality proposals from the broad fusion community to run experiments, taking advantage of the unique capabilities of the major U.S. facilities to address forefront issues in fusion science and technology.

- An Alternative Concepts Review should be held, including inertial confinement fusion, to prioritize approaches and determine a reasonable, healthy, and productive funding range for each in the context of the goals of the restructured fusion program and the FY97 President's Budget Request. An additional product of this review should be a recommendation for an ongoing mechanism for evolving the priorities and balance of confinement concept development (inclusive of all concepts, including tokamaks) and for recommending action on specific proposals from specific groups, consistent with the principle of "due process."

- If the FY97 budget request is less than $250 million, a process to consult again with our international partners to establish a mutually agreeable and potentially revised role in the ITER EDA will be urgent.

- The U.S. ITER Home Team should move aggressively to strengthen its outreach to the entire domestic fusion community. In addition, it is timely to plan for broad U.S. participation in the review of the ITER EDA and its results and to establish criteria for a decision on the level and nature of U.S. participation in ITER construction, if the international partners decide to go forward. Regardless of the decision on ITER, international collaboration represents the best path for the United States to obtain energy generation from fusion. Appropriate U.S. participation in international initiatives and alignment of the domestic program, including technology development, to complement the foreign and international effort are areas requiring fusion community input and consensus building in both the immediate and longer term.

- An evaluation should be initiated to determine the projected cost and benefits of the international 14 MeV neutron source, which is being considered for construction to test and develop low-activation fusion materials. During this evaluation, the budgetary restrictions now limiting the U.S. fusion program must guide any consideration of cost-sharing. Members of the materials research community should continue to use lower cost options for low-activation research, such as sample radiation using fission sources.

- The Subcommittees did not assess the small, accelerator-based effort addressing Inertial Fusion Energy funded by the Office of Fusion Energy, but acknowledge its potential as a fusion energy source and recognize that the major scientific and plasma physics issues are being addressed through DOE Defense Programs, as a component of stockpile stewardship. A review should be conducted, involving all cognizant DOE program offices and appropriate scientific and technical experts, to recommend the priority and management of IFE, in the context of the mission, policy, and scientific goals of the restructured program.

- A vigorous outreach effort should be initiated, with the goal of broadly communicating the goals and progress of this important effort to the public, to the broader scientific community, and to stakeholders, such as the energy industries and environmental groups.

The results of the above actions and the recommendations of the reviews must be integrated into a coherent and balanced program plan for FY1997 matched to the budget level. In addition, more permanent and streamlined governance mechanisms, encouraging continued community and stakeholder input, must be put in place to guide program priorities and evolution into the future.

6 Assessment Summaries Prepared by the Scientific Issues Subcommittee (SciCom)

The Scientific Issues Subcommittee (SciCom) was asked to provide scientific assessments of key areas strongly influencing the direction of the restructured Fusion Energy Sciences Program. These assessments together with joint meetings between SciCom and SPS helped to motivate our conclusions and recommendations. The full text of these assessments are attached as appendices. In some cases, these appendices also illustrate how the implementation of our new policy goals can strengthen key research areas and help set budget priorities. In this chapter, executive summaries, prepared by SciCom, are presented to serve as a guide to the appendices.

6.1 Fusion Program Scientific Goals: Two general goals follow from the mission statement of the U.S. Fusion Energy Sciences Program (see p. 3):

1. Science — develop the core science of plasma physics; produce, understand and optimize fusion plasmas.

2. Energy — develop an attractive electric power producing system from fusion plasmas.

The suggested science objectives for the restructured U.S. fusion program follow (key elements, not in priority order):

- ***Promote plasma science:***

  Reinvigorate OFE leadership of U.S. plasma science.

  Enhance understanding of fusion plasmas.

- ***Push fusion innovation***

  Focus tokamak effort on improvements and demonstrate their potential.

  Advance state of alternative concept research.

  Develop and implement some enabling technologies.

  Develop low activation materials.

- ***Study burning plasmas:***

  Develop a viable U.S. role in an internationally developed, constructed burning plasma experiment.

  Contribute to a successful completion of the ITER EDA.

These objectives are in accord with the priorities set forth in the PCAST report and are amplified in subsequent appendices.

6.2 Development of Basic Plasma Science: The underlying core science of fusion energy is plasma science: the study of the ionized states of matter. The containment of high temperature plasmas required for the production of fusion energy was (and remains) the primary motivation for the DOE/OFE to strongly support the development of the field of plasma science. As stated in the PCAST Report, "This [fusion] funding also sustains an important field of research - plasma science - in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable to other fields of science and in industry."

Further progress in the development of fusion energy will require continuing developments in the field of plasma science. Fusion program support for plasma science provides the primary interface between work in fusion energy and the scientific community. It is an important vehicle for extending the impact of advances in fusion energy to the wide array of interdisciplinary fields supported by plasma science (as documented in detail in the recent NRC Plasma Science Report).

However, Congressional policy direction to the magnetic fusion program (Energy Policy Act of 1992) called for a schedule-driven energy development program in an era of reduced budgets. In response, the program reduced the breadth of support for plasma science.

For these reasons, a key goal of the restructured fusion program is to expand support for fusion science with basic plasma science as a key element. To achieve this goal, the fusion program should explicitly assume the responsibility to advocate and act as a steward for basic plasma science. While other agencies of the federal government provide limited support in this area, the fusion program is the primary beneficiary of advances made in the field of plasma science. The fusion program should take the lead by establishing a program to support basic plasma science, while continuing to work with other federal agencies to provide additional support for more fundamental plasma science research.

The expected benefits to the fusion program from taking on this new responsibility include aiding the development of fusion energy through advances in fundamental understanding of the behavior of high temperature magnetized plasmas and an improved interaction with related disciplines in the scientific community.

An effective program to broaden the plasma science activity supported by the fusion program would require building up to a support level of about 5% of present fusion funding. Some of the mechanisms the fusion energy sciences program could use to implement this program in plasma science are as follows:

1. Take the lead and work with other funding agencies to establish a "critical mass" plasma science research effort in a larger fraction of the nation's research universities;

2. Incorporate plasma science in the structure of the program office in a manner that provides visibility and supports the unique character required to carry out the plasma science program mission;

3. Support cutting-edge theory and experiments that might contribute to the long term rather than only the short-term development of fusion energy;

4. Seek proposals for fundamental plasma science experiments which can be performed on major tokamak facilities in analogy to outreach programs on the University of Rochester Omega laser fusion facility;

5. Establish a Plasma Science Young Investigator Award program to stimulate appointment of and provide support for new university faculty researchers in plasma science modeled after the successful programs in the NSF;

6. Broaden the academic base in the field through outreach to institutions not currently supporting plasma science;

7. Support undergraduate programs in plasma science research and education.

While the need for establishing this program is immediate, success will require a sustained effort by the fusion program to establish the necessary research infrastructure in plasma science. Adoption of the mechanisms suggested above would also be a significant step by the federal government in implementing the recommendations of 1995 National Research Council Report on Plasma Science.

6.3 Theory and Computation: Theory and computation, in conjunction with experiment, provide the predictive capability at the core of the scientific research endeavor. Great progress in the understanding of plasma and fusion physics has been made in recent years (e.g., turbulence, stochasticity and chaos, magnetic reconnection, wave-particle interactions).

Theory is cost effective and has high leverage. Quantitative predictions of plasma behavior are becoming increasingly accurate and contribute to the effectiveness of the experimental program. Progress in the experimental program has produced measurements of more theory-relevant quantities (such as current density profiles and core turbulence, for instance), which in turn uniquely guide the theory effort towards a more realistic description of the plasma. This impetus should be maintained.

The United States remains a world leader in theory and computational modeling. We are well positioned to contribute to and benefit from the worldwide fusion effort.

Even though the U.S. theory program has been effective and successful in the past, with its expanded scope and with an expected increase in computational activity, the theory program will be hard pressed to meet its objectives even if budgets remain steady. In consequence, we point to some recommendations that would improve the present status.

The restructured program will support fundamental research in both plasma and fusion science. This enhances cross-fertilization and scientific visibility, stimulating greater topical diversity in fusion research.

Increased coordination and flexibility in applied theory and computation, based on scientific goals, should be established at both the OFE level and within and among institutions. Evaluation and implementation structures may need re-alignment.

The adequacy of computational resources needs to be assessed and provided as needed. Expected hardware and algorithm optimization developments should be exploited whenever possible.

On the educational level, the new program encourages a broad treatment of plasma science in undergraduate and graduate courses, including non-fusion basic plasma physics subjects such as low temperature and non-neutral plasmas. This will help attract young talent into the field and provide better postgraduate opportunities for young scientists.

Targeted support for valuable young scientists needs to be provided to ensure renewal.

Fostering communication paths within the fusion program and with the broader scientific community will make the whole fusion program more effective. This calls for a diversity of approaches, including expanding the scope of existing meetings, promoting goal-oriented working groups and task forces, and nurturing diversity.

6.4 Major Tokamak Facilities: The U.S. fusion program has three major operating tokamak facilities, the Tokamak Fusion Test Reactor (TFTR), the DIII-D tokamak, and the Alcator C-Mod tokamak. These facilities all have strong scientific teams and provide a strong collection of hardware capabilities for tokamak and basic plasma physics research. Their scientific programs contribute in a major way to the primary U.S. goal of tokamak concept improvement and contribute stimulating ideas and results to the world fusion program. They are centers of collaboration for both U.S. and foreign scientists, providing opportunities to test new ideas and to advance fusion science. They provide a basis for international collaborations, such as scientific personnel exchanges, joint experiments, and contributions to the physics basis for ITER. These are world-class facilities with outstanding records of scientific accomplishment.

A primary goal of the major tokamaks is to understand the plasma science of high-temperature, magnetically-confined plasmas, with concept improvement as the focus of "a strong domestic core program in plasma science," as identified by PCAST. The concept improvement goals for tokamaks are to establish the scientific foundations for steady-state operation, low frequency of disruption, and high power density operation. In the long term, these improvements could significantly enhance the attractiveness of tokamaks, by making them less expensive and more reliable. The scientific issues in this area include the understanding of plasma stability at high pressure, plasma transport, plasma profile control, non-inductive current drive, power and particle exhaust, and alpha particle effects. The highly nonlinear coupling among these elements makes their integration a scientific challenge.

Concept improvement is an exciting and dynamic research area, in which scientific progress worldwide has been rapid in the last few years. The U.S. tokamaks have led this area of research and contributed greatly to its progress.

The three facilities together comprise the largest element of the U.S. magnetic fusion program (about 40% of the total magnetic fusion budget). As a matter of good practice, we consider it essential that any operating experiment be supported with healthy funding to operate cost-effectively. It must have the resources (operating time, hardware upgrades, and scientific staff in appropriate balance) to be fully productive. All three major tokamaks contribute to and advance the goals of the restructured U.S. fusion science program. Each is well positioned to make further scientific advances. However, restructuring the fusion program within budget levels that are greatly reduced from previous years may make it necessary, in the near term, to retire one of the major tokamak facilities. The program of deuterium-tritium plasma studies currently under way in the Tokamak Fusion Test Reactor (TFTR) can be completed in the relatively near term, whereas the programs on DIII-D and Alcator C-Mod extend for a longer term. It is appropriate, then, that TFTR should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from this facility.

The combination of constrained budgets with rapid shifts in program directions may necessitate a premature termination (i.e., within a period significantly less than two years) of the TFTR program. If this is required, a number of important research objectives requiring deuterium-tritium plasmas would not be completed. It is unclear when these lost scientific opportunities would return. For this reason, we believe that sufficient resources should be provided to operate TFTR at high productivity throughout FY97, while also operating DIII-D and Alcator C-Mod at high productivity. In any event, the DIII-D and Alcator C-Mod programs should be supported for full productivity after TFTR is retired during FY98.

6.5 Plasma Confinement Research (Alternative Concepts): Different approaches to magnetic configurations of plasmas can provide a variety of research vehicles to investigate basic plasma physics as well as physics issues common to the magnetic confinement approach to fusion. The term "alternative concepts" refers to magnetic confinement configurations other than the standard or advanced tokamak that is the focus of the worldwide tokamak program. The division of fusion research into mainline tokamaks and alternatives is historical and problematical. It understates the strong physics connections between most magnetic confinement approaches, and the research techniques which they share. It also does not convey the greatly differing stage of development of tokamaks and non-tokamak plasma confinement approaches to fusion. Ultimately, a fusion reactor will likely draw on the broad-based physics foundation that comes from experimental and theoretical studies in a variety of plasma confinement approaches including "alternative concepts."

A prime reason for broadening the scope of the program to include studies in alternative concepts is that the study of more than one plasma confinement system configuration advances plasma science and fusion technology in ways not possible in one system only. Examples of past discoveries and innovations of significance to tokamaks and physics in general are numerous (including discovery of the bootstrap current, invention of helicity injection current drive, development of neutral beam heating, discovery of the dynamo effect in the laboratory, to name a few). For each alternative concept there are challenging scientific issues to understand and resolve. Understanding these issues may lead to improved concepts for reactor applications. Indeed, with decades to go to fusion power, it would be premature to narrow to one concept. Finally, broadening the scope of the magnetic confinement fusion program to include alternative concepts opens the plasma research community to new ideas, an important feature in attracting new talent to the field.

Of the large number of known alternative magnetic confinement concepts, several are arguably ready (in terms of present scientific understanding) for theoretical evaluation and modest-scale experimental studies that might yield important scientific insights into plasma confinement. These include the field reversed configuration, the reversed field pinch, the spherical tokamak, the spheromak, and the stellarator. There have been substantial advances, both worldwide and in the residual activity in the United States, in the theoretical and experimental understanding of various alternative configurations since 1990 (the time at which the alternative concepts research program was nearly eliminated in response to budget pressure).

Reinitiation of an alternative concepts research program will increase the breadth of plasma research and the emphasis on science and innovation, consistent with an increased emphasis on basic plasma research. The resulting diversity will increase the visibility and impact on the larger scientific community. Under the constrained budgets anticipated in coming years, alternative concepts is an area in which the United States can maintain excellence in the world context, with modest expenditure. The science program carried out on alternative confinement concepts should be closely integrated with the tokamak program, recognizing the universality of the physics issues and increasing the attention to underlying science issues.

Existing alternative concept research is already strongly coupled to the international effort. Japan and Europe have large programs in alternatives. They are mainly concentrated on the stellarator, with other concepts pursued mostly in experiments of modest to medium scale. The United States can be at the forefront of research in selected areas of alternative concepts research. If experiments in an alternative concept should prove very successful, such that a large experiment is needed for further progress, then such an endeavor could be pursued internationally, in analogy with research in tokamaks.

We recommend that the concept improvement program be expanded to include a spectrum of alternative concepts plasma confinement systems, including experimental and theoretical research. This is consistent with the report of the PCAST and the Office of Fusion Energy draft strategy, both of which strongly endorse alternative concepts research and include it as part of their highest priority goal. Several concepts may be ready for experiments to elucidate key physics issues. The precise funding level for alternatives cannot be prescribed here. It must be driven by peer-reviewed proposals (from national labs, universities, and industry), as for any scientific research program. As for the program's major facilities, any experiment which is operated should be supported with healthy funding to operate cost-effectively.

6.6 Inertial Fusion Energy: In Inertial Confinement Fusion (ICF) the fusion energy is released by imploding a spherical small pellet of deuterium and tritium using energetic lasers or particle beams as drivers. Supported primarily by Defense Programs in DOE ICF is now the largest fusion program in the U.S. The principal purpose of the ICF program is stockpile stewardship to provide the scientific base for nuclear security applications. The National Ignition Facility (NIF) is an approved billion-dollar ICF facility designed to demonstrate ignition in ICF pellets by about 2005. Study of the hot spot and burn in NIF will also settle the main scientific issues of high-gain targets and establish the driver requirements.

Four different drivers have been identified and studied for ICF purposes: glass-based lasers, KrF lasers, light-ion accelerators, and heavy-ion accelerators. The development of the first three drivers was funded by Defense Programs, and glass-based lasers were chosen for NIF. Heavy-ion accelerators have been investigated for Inertial Fusion Energy (IFE) power applications, because for these applications the drivers must be reliable and efficient with a high pulse-repetition rate (several Hertz) and long life.

Many recent reviews of the IFE program have indicated that accelerator development is ready to proceed to the next step - - the Induction Linac Systems Experiment (ILSE) project, which would provide an integrated demonstration of induction-linac technology and the beam physics required to provide the data base for scaling to a heavy-ion driver for an inertial fusion power plant. In 1993 the Fusion Energy Advisory Committee Panel 7 on IFE reviewed the status of IFE efforts in the Office of Fusion Energy and recommended a "balanced program that includes an experimental and analytical program for supporting IFE technologies as well as accelerator development and beam physics." This panel recommended a "reference" annual budget of $17M, with $14M for ILSE and accelerator research, and $3M for supporting technology and systems studies. The panel also found that for $10M per year it is not possible to complete the ILSE project, although a significant set of accelerator experiments could be conducted to increase understanding of key technical issues.

At an annual budget of $8M, IFE cannot proceed to its logical next step (ILSE). This budget reflects the limited scientific synergy between accelerator development and magnetic fusion (MFE). However, IFE and MFE share a large number of scientific issues: MFE plasma science and IFE driver-independent plasma science issues (funded in Defense Programs), and the fusion-support technologies of both IFE and MFE.

The Subcommittees did not assess the IFE effort in detail, but acknowledge its potential as a fusion energy source and the major role of DOE Defense Programs in addressing key scientific and plasma physics issues. A programmatic review should be conducted involving all cognizant DOE program offices and appropriate scientific and technical experts to recommend the priority and management of IFE, in the context of the mission, policy, and scientific goals of the restructured program.

6.7 International Thermonuclear Experimental Reactor (ITER): The logical follow-on to the experimental program being carried out in the present generation of tokamaks is a burning plasma experiment, that is, the demonstration of controlled ignition and extended burn of a DT plasma. Goals for a burning plasma physics experiment include both the demonstration of long-pulse ignition, and the demonstration of a driven, high-beta, high bootstrap-current-fraction steady-state burn. This latter goal represents the continuation of the concept innovation program being pursued at our large tokamak facilities. The new physics issues that must be addressed on such a facility mainly relate to fusion alpha physics, dynamic control of the operating point, control of plasma profiles (through non-inductive current drive and the formation and control of transport barriers), particle and heat removal, and disruption avoidance and/or mitigation.

Burning Plasma Physics will be pursued through international collaboration. At present, the principal international collaboration aimed at the construction of a burning plasma experiment is the ITER EDA — a collaboration involving the United States, the European Community, Japan, and the Russian Federation that seeks to demonstrate the scientific and technological feasibility of fusion power. The ITER mission addresses both the physics and technology issues essential to an engineering test reactor. ITER is both a great physics challenge (if ITER is built, it will test burning plasmas at the reactor scale and provide a test bed for most of tokamak physics, and much generic physics) and a great technological challenge (it requires the development of high-field superconducting magnets, high-heat-flux components, plasma heating and fueling systems, and other reactor-relevant fusion technologies). U.S. industry has been given a major role in designing and building prototype components of the ITER during the EDA, while having access to all design and development activities of the other parties. This role helps to assure that

American industry will be able to compete for construction elements, if ITER is built and the United States participates. In addition to the potential benefits of sharing the ITER construction cost, this important international collaboration focuses the world fusion program on a concrete objective.

The U.S. program extracts substantial benefits from the ITER EDA. At present, ITER is the primary vehicle for plasma technology development in the U.S. program (as discussed in the technology section below). ITER Physics R&D also provides one of the focuses for our large tokamak experiments. Important physics issues must still be resolved in the areas of divertors, disruptions, density limits, transport scaling, L-to-H and H-to-L mode power thresholds, current and pressure profile control, and beta-limits. The ITER EDA has added urgency to the international effort to address these issues. The results of this physics R&D, together with the ITER physics design requirements, will form the physics basis for ITER. This ITER Physics Basis must be assessed by all parties before there is a decision to construct ITER. In particular, the U.S. program will review the ITER EDA output (including both physics basis and engineering design) prior to a decision to seek participation in ITER construction.

Given the pivotal nature of a decision on ITER construction to the U.S. program, we recommend that a mechanism be established immediately to expand involvement of the U.S. fusion community in the assessment of the evolving ITER physics and technology basis to ensure that the ITER design reflects our current best understanding of tokamak physics and to ensure that the U.S. community appreciates the issues that have driven ITER design decisions.

The recent budget changes in the U.S. fusion program indicate that the United States is very unlikely to participate as a full partner in ITER. Nevertheless, we recommend that the U.S. program continue participation (as allowed by the budget) in the EDA phase of ITER to which the United States is committed through FY98, thereby fulfilling our existing commitments to our international partners and leaving open the possibility of some U.S. participation in ITER construction or other major international collaborations which provide a cost-effective means of advancing fusion science.

In the remainder of the EDA, it is important for the U.S. program to increase its emphasis on advanced tokamak scenarios to ensure that the ITER facility provides a suitable vehicle to pursue the study of tokamak concept innovation in a burning D-T plasma. This effort would include increased attention to operational flexibility, the definition of the ITER heating and current drive systems, diagnostics, and control systems. These are areas in which the U.S. program might have maximum impact at minimum cost during ITER construction and operation.

Regardless of the outcome of the post-EDA phase, the United States will have benefited from our involvement in the ITER EDA, because ITER has acted as a driver for technology development, for engineering design innovation, and for involving the world in an enhanced collaborative attack on the primary physics and operational issues of tokamaks. It has also forced the fusion community to face squarely the engineering challenges of designing a steady state, high power, D-T burning plasma device.

In 1998 the EDA agreement will be concluded, and the parties will be faced with a decision about ITER construction. Since ITER construction is expected to cost in excess of $6B, clearly the U.S. program cannot participate as an equal partner at present budget levels. However, the European, Japanese, and Russian programs have indicated that they may welcome U.S. participation as a limited financial partner. Hence, if our ITER partners agree to pick up the bulk of the cost of ITER construction, and the U.S. program agrees that the ITER design complements our goals, then we should seek to participate in ITER construction as a limited financial partner to provide a continuing focus for our tokamak physics and technology program, so that U.S. scientists will have access to the ITER facility after it is completed, and so that the United States can benefit (to an extent commensurate with our contributions) from lessons learned in ITER construction and operation. Possible U.S. contributions to ITER construction include engineering and component production, as well as physics design. As an example, a focus on plasma diagnostics and control systems would lead naturally to the provision of a remote ITER access site in the United States for our participation in ITER operation.

A low-level U.S. domestic effort to search for less expensive means of studying burning plasmas would be useful insurance against the possibility that ITER is not constructed. Note that any such effort could not be part of the U.S. contribution to the ITER EDA. Should that search prove successful and in the event the international partners decide to modify their objectives for a next-step device, then the United States should explore with its former ITER partners (and other nations if this becomes appropriate) the possibility of international collaboration on a less expensive means of fulfilling the goal of DT ignition and burn.

6.8 Fusion Materials and Technology: Fusion science encompasses fundamental materials and enabling technology development, including (1) low activation materials and fusion technologies essential to achieve safety and environmental potential of fusion and (2) critical plasma technologies required to support advances in plasma physics. The science associated with fusion materials and technology development acts as a driver for a wide range of scientific applications far beyond fusion (e.g., superconducting magnets, high temperature and radiation resistant materials, cryogenic materials, electromagnetic power systems, heat removal systems, and plasma diagnostics).

The development of low activation materials is a long-term endeavor and a critical issue on the path to fusion energy. The performance requirements for materials are unprecedented. Materials must withstand 14 MeV neutron irradiation damage, high operating temperatures, thermal and mechanical loads, and chemical compatibility requirements. The materials science associated with these requirements must be understood to determine performance and lifetime limitations of candidate materials with low activation characteristics. The structural materials effort is currently limited to only three candidate materials: vanadium alloys, SiC composites, and ferritic steels. In addition, development of low activation materials for non-structural applications (e.g., plasma facing materials, designs and developments, electrical insulators, tritium breeding) is critical for fusion.

Fission reactors are currently used for irradiation studies. However, a 14 MeV neutron source will ultimately be needed to fully qualify materials for fusion reactor applications. Conceptual development of a materials test facility is being pursued as part of an international collaboration. A cost estimate for this facility is expected to be available in May 1996. Since preliminary information suggests that the cost will approach $1B, it is difficult to see how the U.S. share of this facility cost could be accommodated within the present budget.

The U.S. low activation materials program must address this problem over the next year. Possible resolutions include substantial down-sizing of the facility, participation as a limited financial partner, or seeking funding outside of the fusion program. In any event, the low activation materials program should be continued with extensive international collaborations.

The safety, environmental attractiveness, and economic competitiveness of fusion will depend to a large extent on the blanket system, since this is the largest component exposed to a high neutron fluence. Development of a blanket system that meets performance requirements of tritium self-sufficiency and efficient energy recovery, while meeting the safety and environmental goals of fusion, remains a critical issue. This technology also includes tritium and safety-related technologies. A fundamental understanding of key materials and technology issues is required to develop a compatible combination of materials (breeder, coolant, and structure) for the blanket system. Key issues include chemical compatibility, neutronics, thermohydraulics and stress analysis in addition to the irradiation effects and tritium interactions.

The U.S. program should focus on those issues which are: critical to the safety/environmental goals of fusion; lead in areas in which the United States has demonstrated particular expertise; and make extensive use of international collaboration. To be an effective participant in international collaborations the United States must maintain a significant base program in fusion technology. In addition, it is important that the low activation structural materials program be closely associated with the blanket technology effort.

Plasma technologies provide the enabling capabilities for near term and future plasma science and fusion energy applications. Plasma and enabling technologies for fusion include the following:

- Superconducting magnets
- Steady state heat and particle control components
- Plasma heating and fueling systems
- Plasma diagnostics

The application of new plasma technologies has been essential to the advancements in fusion. In the 1960's hot plasmas were of short duration with many impurities, and the state of the vacuum vessel walls was not characterized. During the 1970's auxiliary heating systems provided controlled multi-kilovolt plasmas. More recently, high field copper and superconducting magnets, improvements to fueling, choice of plasma facing materials and wall conditioning techniques have led to significantly higher temperatures and longer pulse lengths in tokamaks and other configurations.

ITER is now the primary vehicle for plasma technology development in the U.S. program. The United States has the lead in two of the seven major ITER deliverables — the central solenoid model coil and the divertor cassette prototype — and plays an important role in the shielding blanket module, vacuum vessel sector, and remote handling demonstrations. The ITER R&D is highly leveraged, since all parties are contributing to these deliverables in a coordinated program. We obtain all of the R&D results for less than one third of the cost.

Any current or next generation device will benefit from plasma and enabling technology R&D. The superconductor and fabrication concepts adopted by ITER originated in the U.S. program.

This basic concept of the "cabled" niobium-tin superconductor is embodied in the design of superconducting fusion devices. Plasma-material interactions are intrinsic to all magnetic fusion concepts. In addition, compact advanced tokamaks and alternative concepts will require enhanced heat removal technology due to their higher heat fluxes. The United States is a leader in developing actively cooled plasma facing components. Heating and fueling systems are required for any magnetic confinement concept. In electron cyclotron heating, the United States holds the world record for pulsed gyrotron energy output and CW operation. U.S. ion cyclotron heating antennas are used on fusion devices around the world. The United States is preeminent in pellet fueling technology. Finally, innovative U.S. plasma diagnostics also provide a convenient means for obtaining international collaborations in foreign facilities.

Regardless of the longer term role of the United States in ITER construction, further investments in plasma technology must be made to insure optimal advancement of plasma science in the future. These activities may be undertaken both nationally and internationally. Industry will continue to play a valuable role, especially when a particular technology comes from applications outside of fusion, so that fusion can leverage off of a broader support.

Attachment 1

Department of Energy
Washington, DC 20585

December 5, 1995

Dr. Robert W. Conn, Chair
Fusion Energy Advisory Committee
School of Engineering
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0403

Dear Dr. Conn:

In 1991, the National Energy Strategy (NES), following a recommendation of the Fusion Power Advisory Committee, defined the principal goal of the fusion energy program as "Prove fusion energy to be a technically and economically credible energy source, with an operating demonstration plant by about 2025 and an operating commercial plant by about 2040."

Since then, program funding has been inadequate to pursue the goal defined in the NES. Recognizing this situation, as part of the FY 1995 budget deliberations, Congress urged that a review of the fusion program be undertaken by the President's Committee of Advisors on Science and Technology. The review was completed in July 1995 and the Committee agreed that, while the DOE fusion strategy was reasonable and desirable, it was unrealistic in the current climate of budgetary constraint. The Committee recommended a program strategy funded at $320 million for each of the next ten years to preserve the most indispensable elements of the U.S. program and associated international collaboration. The Committee further stated that it was unable to envision a program that could preserve the key program priorities at a budget of $200 million per year.

Congress has now passed, and the President has signed, an appropriations bill providing $244 million for fusion, a reduction of almost one third below the FY 1995 level of $357 million. In the Conference Report accompanying that bill, Congress stated that there is "...little prospect for increased funding for the fusion base program over the next several years...," and we have had indications that funding might well be reduced further. Congress also directed the Department, working with the fusion community and the Fusion Energy Advisory Committee (FEAC), to prepare a strategic plan to implement a restructured program and

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provided some direction regarding the content of a restructured program. A copy of the Congressional language is enclosed (Enclosure 1).

As a first step, the Office of Fusion Energy, in concert with members of the community, has developed a draft strategy, defining a new mission for the fusion program. The reductions in current and projected fusion funding preclude us from implementing a program based on the goal set in the NES. The draft plan therefore redefines the fusion program from a schedule-driven, energy technology development program to a budget-constrained research effort aimed at reducing the size and cost of fusion reactors and advancing scientific knowledge on key fusion issues. The proposed mission of the new program is to establish a scientific knowledge base from which a fusion energy technology development program could be undertaken in the future, when appropriate. The draft strategy is also enclosed (Enclosure 2) for your review and comment, and Dr. N. Anne Davies will brief the Committee on it during your meeting.

In its deliberations on the restructuring of the fusion program, I want the Committee to consider the broader issue of plasma science that underpins fusion energy and has numerous applications in science, technology, and the commercial sector. Given the recent National Research Council report (Plasma Science from Fundamental Research to Technological Applications) recommendation that plasma science should receive increased funding, give us your views on how an expanded plasma science program might fit into a restructured fusion energy program.

In addition to your comments on the general strategy for the program, we need the Committee's help in planning the implementation of that strategy, including institutional considerations and the role of ITER and other international collaboration in the program. I would like FEAC to establish a Strategic Planning Subcommittee to assist in these tasks. Both FEAC and its Strategic Planning Subcommittee will require an unbiased assessment of the technical capability of the major U.S. fusion facilities and what they are able to contribute to the program's priority issues. FEAC will also require scientific assessments, based on analyses of experimental results, theory and modeling free of institutional bias, to inform future deliberations. I also want you, therefore, to establish a Scientific Issues Subcommittee, which will continue in existence beyond this particular charge, to provide scientific assessments that will inform the Committee's future deliberations, as well as the current ones.

I would like to have your recommendations on how to restructure the fusion program by mid-January. A set of questions to guide your deliberations is enclosed (Enclosure 3). I recognize that this is a very tight schedule but we need your advice in order to incorporate the restructured program in our FY 1997 budget request to the Congress. I look forward to meeting with you and the other FEAC members, and appreciate your willingness to address this important subject over such a short period of time.

Sincerely,

Martha Krebs

Martha A. Krebs
Director
Office of Energy Research

Enclosures

Enclosure 1

Fusion Section of the Conference Report for the FY1996 Energy and Water Development Bill Congressional Record - October 26, 1995

Fusion

The conferees have provided $244,144,000, an increase of $15,000,000 over the House recommendation, for the fusion energy program. This funding is to support a program in plasma science and fusion technology, and continue United States participation in the engineering design activities phase of the International Thermonuclear Experimental Reactor project to which the United States is committed through fiscal year 1998. The conferees do not agree with the Senate language which recommended transferring computer work, termination, severance and separation costs to other activities within the Department, and transferring the heavy ion fusion program to defense activities.

With little prospect for increased funding for the fusion base program over the next several years, it will be necessary for the program to restructure its strategy, content and near-to-medium-term objectives. The restructured program should emphasize continued development of fusion science, increased attention to concept improvement and alternative approaches to fusion, and development and testing of the low-activation structural materials so important to fusion's attractiveness as an energy source.

The Department of Energy, with participation of the fusion community and the Fusion Energy Advisory Committee, is instructed to prepare a strategic plan to implement such a restructured program, to be completed by December 31, 1995. This plan should assume a constant level of effort in the base program for the next several years; as appropriate, it should be integrated with plans of the international fusion program; and it should address the institutional makeup of a domestic program consistent with the funding assumptions.

The conferees believe that, because of the stringent budget realities facing this Nation, the promise of fusion energy can only be realized through international collaboration. The high cost of fusion development points to the increasing importance of international cooperation as a means of designing, building, and financing major magnetic fusion facilities in the future. Because the United States has committed to such an approach, it is crucial that a restructuring of the fusion program maintain a strong domestic base and not undermine our credibility as a reliable international partner.

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Enclosure 2

Strategy for A Restructured U.S. Fusion Energy Research Program

Introduction

The Department of Energy's mission includes the development of fusion as one of the few long-term energy options with virtually unlimited fuel supply and favorable potential as a safe and environmentally attractive energy source. Under the reduced Federal funding envisioned for fusion, the U.S. Fusion Energy Program is being restructured to focus on fusion's underlying scientific foundations, including those technologies needed to enable scientific discoveries, and on fostering improvements in plasma confinement concepts in order to reduce the size and cost of future fusion power plants. Such a focus will also substantially strengthen the field of plasma science - a field in which the U.S. is the world leader and which has developed a variety of techniques widely applicable to other areas of science and technology. This revised strategy which focuses on the underlying science is a substantial departure from the Program's previous schedule-driven strategy aimed at operation of a demonstration power plant by the year 2025.

To accommodate reduced budgets for the foreseeable future, the Program must cut deeply into its considerable investments in human resources and facilities. In pursuing its new strategy, the Program will rely heavily on existing facilities, and will use the leverage offered by international linkages to contribute to and capitalize on the world fusion effort. Continued emphasis on innovation and the intellectual challenge of fusion will nurture the vitality and scientific richness needed to retain and attract the scientists and engineers required to accomplish this task.

Program Mission

Establish the scientific and technological foundations for an economically and environmentally attractive fusion energy source.

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Core Values

The extraordinary challenge of fusion requires major advances in our knowledge of plasma physics, fusion technology and materials science. The development of the underlying fusion science and enabling technology, the foundation for such advances, is the single most important program element. A few examples among many topics requiring progress in understanding are plasma-wall interactions, plasma stability and transport, high-heat-flux alloys, and alpha-particle relaxation. Fusion research of the required quality depends upon: (i) the maintenance of a broad, vigorous talent pool, working in an intellectual environment that encourages creativity and innovation; (ii) effective connections to related scientific and engineering disciplines; (iii) a range of experimental facilities, including small devices for exploratory studies as well as larger facilities capable of approaching the physical conditions of a fusion reactor; and (iv) effective leverage of U.S. efforts through mutually beneficial international linkages.

Strategy

The issues of confinement concept optimization, burning plasma physics, and low activation materials remain fundamental to fusion research; they must be addressed by the restructured Program. The restructured Program will place a greater emphasis on concept optimization and a lesser emphasis on burning plasma physics, building on the significant accomplishments and valuable resources of the scientific program. Efforts on low activation materials will remain at a modest level compared to the other two. The issue of fusion power technology, including blankets and tritium handling, is also fundamental and must be addressed in the future as part of a fusion energy development program; this issue will not be addressed by the restructured Program except for small-scale efforts in selected key areas.

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The three fundamental issues, associated objectives, and the approaches to addressing these issues in the restructured Program are discussed below. The objectives, while bold and challenging, can be pursued at a lower level of resources than was required for achieving the more ambitious goals of the previous Program strategy.

- Confinement Concept Optimization: Establish the scientific basis for one or more promising plasma confinement approaches that could lead to a significantly simpler, less expensive, and more reliable fusion energy source than one based on the present data base.

  Recent advances in understanding and performance that have been made with both tokamak and alternate plasma confinement approaches have illustrated the potential for major improvements in the attractiveness of fusion energy sources. The realization of this potential requires a broad-based fusion science and enabling technology program that fosters innovation and creativity. A two-pronged approach will be pursued: research to enhance the performance and attractiveness of tokamaks, conducted primarily through reliance on existing facilities, and expansion of the exploration of promising alternate plasma confinement approaches. Where appropriate, program activities will take into account the substantial fusion research investments being made and the innovative work being undertaken on alternate confinement approaches in Europe, Japan, and Russia. In addition, inertial confinement fusion is being pursued in the Department's Defense Programs, with a relatively small research effort on energy-specific enabling technology in the Fusion Energy Research Program.

- Physics of Burning Plasmas: Establish the scientific basis needed to understand and predict the behavior of burning plasmas under conditions relevant to a fusion energy source.

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Exploring the many physics aspects of optimizing and sustaining burning Deuterium and Tritium (i.e., D-T) plasmas has been a major objective of the Program. In the near term, it is possible to address this objective experimentally for short pulses and moderate levels of alpha particle production in existing D-T devices. Important physics issues will also be addressed in the near-term by theory and by devices operating with plasmas that, while not D-T, simulate some of the phenomena in burning D-T plasmas. Given the constrained budget outlook for the Program, exploration of the physics of burning plasmas for long pulses and substantial alpha particle production can only be met through collaboration in broader international activities. As part of this, the Program will continue to participate in the International Thermonuclear Experimental Reactor (ITER) Engineering Design Activities (EDA). The Program will also seek to participate, even at a modest financial level, in the construction and operation of an international D-T plasma burning device as now embodied in ITER, in order to explore more thoroughly the physics of burning plasmas at high energy gain.

- Low Activation Materials for Fusion: Establish the feasibility of using low activation materials to significantly enhance fusion's potential as a safe and environmentally attractive energy source.

  The materials surrounding a burning plasma must function in a demanding environment which includes high heat fluxes, substantial mechanical loads, and intense neutron bombardment. Development of compatible first-wall and blanket materials with low-activation characteristics is essential if fusion is to realize its full potential as a safe, economical, and environmentally attractive energy source. Because development of materials for the fusion environment requires a basic understanding of materials behavior under a

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combination of severe operating conditions, this is recognized as a long-term undertaking. This objective will be accomplished through strong international collaboration on materials development based on fundamental research. It is expected to include a modest level of participation in an international materials testing facility to gain knowledge about the performance of materials in a characteristic fusion neutron environment.

Conclusion

In this revised program strategy, the focus of the U.S. fusion program has shifted from the operation of a demonstration power plant in 2025 to fostering improvements in confinement concepts and the underlying science and enabling technology of fusion. Human resources exist within the Program to support the new strategy and to contribute toward the world's fusion effort in many areas of critical need. The Program will require a range of national experimental facilities, from existing and new devices for exploratory studies, to larger facilities that can approach the operating conditions of a fusion energy source. Existing experimental facilities can address many of the Program objectives and provide information required for successful completion of the ITER EDA. The Program will continue to be an effective participant in the ITER EDA and will attempt to participate, even at a modest financial level, in the construction and operation of an international D-T plasma burning device as now embodied in ITER. Continued development of enabling technologies, support for theory and computational efforts, system studies, and use of international collaboration will permit the restructured Program to realize its vision.

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Enclosure 3

Questions for the Fusion Energy Advisory Committee

What major science, technology, and policy goals should shape the U.S. fusion R&D program if it is funded at $200, $225, $250, and $275 million for the foreseeable future? In formulating your response, the Committee should consider the fusion research and development programs of other nations.

For each of these budget cases:

- Does the proposed strategy that has been developed by the Department and the fusion energy community meet these goals? How would the program be optimized if funded at each of these levels?
- What R&D elements would the Committee recommend preserving within a restructured fusion program?
    - What special capabilities do we have that should be emphasized to stay at the forefront of this or related fields?
    - What are the areas of strength in the U.S. fusion program which should be emphasized in order to maximize our attractiveness as a partner in international collaborative activities?
- What is the appropriate level of U.S. participation in the International Thermonuclear Experimental Reactor project?
- What should be the balance of research and technology effort among universities, federal laboratories, and industry within a restructured program? Should the program consolidate its activities among fewer performers? If so, how should the program go about doing this?

Appendices

The Scientific Issues Subcommittee (SciCom) of the Fusion Energy Advisory Committee assisted the review of the fusion program by providing scientific assessments of major issues covering:

A. Fusion Program Scientific Goals

B. Development Of Basic Plasma Science

C. Theory and Computation

D. Major Tokamak Facilities

E. Plasma Confinement Reasearch (Alternative Concepts)

F. Inertial Fusion Energy

G. International Thermonuclear Experimental Reactor (ITER)

H. Fusion Materials and Technology

These scientific assessments are treated in white papers that comprise the following appendices. The white papers were prepared to inform the Strategic Planning Subcommittee (SPS) in its deliberations. Short summaries are included in Section 6 of the body of the report. In cases where there are inconsistencies, the recommendations and conclusions in the main body of the report, especially in Sections 1 through 5 which take into account other factors, take precedence.

Appendix A

Fusion Program Scientific Goals

The **Mission of the Fusion Program** is to:

Advance plasma science, fusion science and fusion technology -- the knowledge base for an economically and environmentally attractive fusion energy source.

The two general goals that follow from this mission statement are:

1) Science -- develop the core science of plasma physics; produce, understand and optimize fusion plasmas.

2) Energy -- develop an attractive electric power producing system from fusion plasmas.

The suggested science objectives for the restructured U.S. fusion program are (key elements, not in priority order):

- **Promote Plasma Science**:
  - Reinvigorate OFE leadership of U.S. plasma science [1].
  - Enhance understanding of fusion plasmas.
- **Push Fusion Innovation** (natural U.S. niche in world program):
  - Focus tokamak effort on improvements and demonstrate their potential.
  - Advance state of alternate concept research.
  - Develop and implement some enabling technologies.
  - Develop low activation materials.
- **Study Burning Plasmas**:
  - Develop a viable U.S. role in an internationally developed, constructed burning plasma experiment.
  - Contribute to a successful completion of the ITER EDA.

These objectives are in accord with the priorities set forth in the PCAST report [2], and are amplified in the following text and subsequent appendices.

A.1 Plasma Physics And Fusion

The fusion program has always been both a science and an energy program -- a use-inspired science program. The core, unique science of fusion is plasma physics or plasma science -- the study of ionized gases in which the effects of collective interactions of charged particles dominate over their binary interactions. Plasma physics embodies most elements of classical physics (mechanics, electrodynamics, kinetic theory, and fluid mechanics). It has practical applications in many areas (plasma processing, particle accelerators, microwave generation, etc.) and it has contributed significantly to diverse fields of science (e.g., nonlinear dynamics, space physics, and astrophysics). Finally, it is a modern science that has been developed primarily through the fusion program over the past few decades. While many fundamental aspects of high temperature plasma physics have been developed, it is still an intellectually challenging, vibrant, growing science -- now particularly in comparisons of theory with experiment.

A.2 Plasma Science

Because plasma physics is the core science for fusion and provides so many spin-offs, it is incumbent on the fusion program, which has historically been the principal driver for the development of modern plasma physics, to assume a leadership role in promoting and supporting plasma physics research. The Office of Fusion Energy (OFE) has historically been the dominant steward of plasma science. However, during the recent past, as fusion funding has decreased and the emphasis was placed on fusion energy development, the OFE support for plasma science has decreased and narrowed. A recent National Research Council report [1] calls for "reinvigoration of basic plasma science," particularly small-scale, university-based basic experimental plasma science. In the restructured fusion program it is recommended that OFE be the primary supporter of high temperature magnetized plasma physics, and related basic plasma studies. In addition, it is recommended that OFE take the lead and work with other federal agencies to increase support for basic plasma science research -- low temperature plasmas (BES, NSF, NIST), high energy density plasmas (DP), space physics (NASA, AFOSR), fundamental plasma physics (NSF), astrophysics (NSF), high density beams and x-ray sources (ONR, DNA), etc.

A.3 Fusion Plasma Physics Issues

A set of key fusion plasma physics issues (adapted in part from the NRC report[1]) are:

1) Magnetohydrodynamic Equilibrium, Stability, and Dynamics (Plasma Control);
2) Transport Processes (Plasma Confinement);
3) Plasma-Wall Interactions (Limiters, Divertors);
4) Wave- and Particle-Plasma Interactions (Plasma Heating, Fueling, and Current Drive);
5) Burning Plasma Physics (Alpha Physics, Burn Control);
6) Composite Issues (Systems Integration).

Many elements of most of these issues can be studied in small to medium scale experiments; the burning plasma physics issues and most composite issues are studied only in the largest experiments in deuterium-tritium plasmas.

A.4 Enabling Technology Issues

Fusion science encompasses not only plasma physics but also many frontier technologies and their related sciences. Various types of technologies are critical for fusion energy:

1) Plasma Technologies -- high power beams of energetic atoms, many precision diagnostics, wall cleaning methods, high frequency microwave generators, hydrogenic ice pellets, compact high power density wave antennas, divertors at the plasma edge, etc.

2) Power Plant Technologies -- superconducting magnets, tritium-breeding and energy-conversion blankets around the plasma (which must simultaneously handle fluxes of high energy neutrons and radiation, and time-varying electromagnetic fields), and remote handling.

3) Low Activation Materials -- to achieve the full safety and environmental potential of fusion it is essential to develop low activation materials to minimize the radioactivity induced in surrounding structures by fusion-produced neutrons and radiation-resistant materials capable of functioning in the fusion power plant environment.

4) Experiment, Power Plant Designs -- design projects seek definitive engineering solutions for given physics requirements. They and power plant systems studies are used to identify critical issues and innovation opportunities in both the plasma and technology areas.

A.5 Recent History

The U.S. fusion program led the development of most fundamental plasma physics concepts and fusion technologies during the 1970s and 1980s. However, because U.S. fusion funding has been reduced by a factor of more than three (in inflation-adjusted dollars) since 1977 as those in Europe and Japan have increased, the U.S. fusion program is now less than 1/6 of the worldwide effort on developing fusion energy. Nonetheless, the U.S. fusion program continues to play a strong, prominent role in many fusion plasma physics and technology areas -- theory, computation, diagnostics, modeling, comparisons of theory with experiment, new modes of experimental operations, non-inductive current drive, plasma-facing components, modern divertors, low activation materials, tritium breeding blankets, etc. The present niche of the U.S. fusion program is science-based innovation in fusion plasma physics, enabling technologies and low activation materials.

A.6 Confinement Concepts

Of the various possible confinement concepts, tokamaks achieved the best plasma parameters early (about 25 years ago) and have continued to lead in this area. Consequently, most of the world's fusion resources have been expended in furthering their development. There remains, however, a strong impetus to develop a better understanding of confined plasmas to reduce risks and increase margins for success in future power plants, and to broaden the scientific underpinnings of the fusion science program. Thus, efforts should be focused on producing "tokamak improvements" with significantly improved performance, and "alternative concepts" (both magnetic and inertial) that offer the prospect of providing more attractive fusion concepts in the long run. Inertial fusion energy (IFE) research is leveraged on a large inertial confinement fusion effort supported by DOE Defense Programs and is the primary alternate to tokamaks in the United States. The Office of Fusion Energy component of inertial fusion (IFE) is presently focused on developing heavy-ion accelerators.

A.7 Innovation Focus

While modest improvements in some elements of a fusion energy system would improve its attractiveness, qualitatively new concepts that offer the prospect of significant improvements would be much more helpful for the ultimate development of fusion energy. Thus, it is appropriate in the current highly constrained budget environment for the U.S. fusion effort to ensure a strong, vibrant and continually developing plasma and fusion science base program that is focused on concept innovation in both tokamaks and alternates (IFE and magnetic). Also, it should foster the continuing development of selected critical enabling technologies and low activation materials in which the United States can play a leadership role.

An emphasis on innovation will reduce program risk in the long run. The U.S. fusion program should also leverage its investments into the much larger world fusion program to the maximum extent possible through international collaborations in areas where the United States can play a significant and innovative role. The key criterion for support should be the largest anticipated fusion science impact per unit expenditure -- irrespective of from which institution(s) or type of institution(s) the proposed research originates, the taxonomy of the confinement concept, or the size of the group or device.

Finally, since the development of fusion is intellectually very challenging, of great interest to students ("combined opportunity to address a fundamental societal need for new energy resources and to engage in research at the frontier of science" -- input to FEAC from current graduate students in the fusion program), and a long term effort (50 years or more into the future before commercialization, at least in the United States), there should be provision for recruiting, educating, developing and retaining outstanding, innovative scientists and engineers for the fusion program in a sustainable manner.

A.8 Burning Plasmas

The study of the key fusion issue of burning plasma physics requires the successful integration of all elements of fusion science and technology into a large, integrated fusion burning experiment. Since the cost of such an integrated facility is in the range of billions of dollars, it has been decided that such a device should be built through international collaboration -- so as to reduce the cost and programmatic risk by any one country. The United States should continue to participate with the world fusion program in developing an integrated burning plasma experiment.

The present focus of this international collaboration is the ITER project, whose six year Engineering Design Activity (EDA) phase is due to be completed at the end of 1998. The United States should continue its participation in the ITER EDA and thereby help to bring about a successful completion of the EDA. This allows the United States to obtain maximum benefit (in the worldwide establishment of the physics and technological basis for a burning plasma experiment) from the ITER EDA, and to facilitate continued U.S. influence on the development of the ITER device. In this continued participation in the ITER EDA process, the United States should focus at least some effort on areas where the United States can expect to be able to participate in an ITER construction project at moderate cost as a limited financial partner and where the United States could play an innovative, leading role -- for example, in diagnostics, control and data acquisition, some key magnet areas (e.g., magnet instrumentation and quench protection), divertor cassette fabrication and assembly. Within the ITER EDA, U.S. participation in any new areas would have to be negotiated with our ITER partners.

A low level U.S. domestic effort to search for less expensive means of studying burning plasmas would be useful insurance against the possibility that our international partners (Europe, Japan and Russia) decide not to construct ITER. Should that search prove successful and in the event that our international partners decide not to construct ITER, then the United States should explore (with its former ITER partners and others) the possibility of international collaboration on a less expensive means of fulfilling the goal of studying burning plasmas.

REFERENCES:

[1] "Plasma Science -- From Fundamental Research to Technological Applications," NRC Report (Nat. Academy Press, Wash., DC, 1995).

[2] "The U.S. Program of Fusion Energy Research and Development," The President's Committee of Advisors on Science and Technology (PCAST) Report, July 1995.

Appendix B

Development Of Basic Plasma Science

B.1 Introduction

Plasma science is the study of ionized states of matter. It includes the core discipline of plasma physics, but as defined in the NRC Report on Plasma Science,[1] it is now a much broader collection of phenomena in ionized matter in which atomic, molecular, radiation-transport, excitation, and ionization processes as well as chemical reactions can play significant roles. The common research themes in plasma science are:

- Wave-Particle Interaction and Plasma Heating
- Chaos, Turbulence, and Transport
- Plasma Sheaths and Boundary Layers
- Magnetic Reconnection and Dynamos

All four of these intellectual problem areas are central to the work being carried out to develop magnetic confinement systems for the high temperature plasmas required for the production of fusion energy. It is through these four research problem areas that the plasma science developed for fusion energy makes connection with the other important fields of application of plasma science including low-temperature plasmas (e.g., semiconductor fabrication), non-neutral plasmas (e.g., atomic clocks), inertial confinement fusion plasmas (e.g., energy production and weapons simulation), particle accelerators and coherent radiation sources, space plasmas, and astrophysical plasmas.

B.2 Role of Plasma Science in the U.S. Fusion Energy Program

Plasma science is central to the effort to develop magnetically confined plasmas as a fusion energy source and the DOE led effort to develop fusion energy has been (and continues to be) the largest driver for the intellectual advancement of plasma science. As stated in the PCAST Report, "This [fusion] funding also sustains an important field of research -- plasma science -- in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable to other fields of science and in industry." Advances in each of the four intellectual problem areas listed above are essential to make further progress on fusion energy development as illustrated with a few examples listed below:

Wave-Particle Interaction and Plasma Heating: Present fusion research in this area is centered on the development of efficient methods to drive locally controllable plasma currents non-inductively with RF excited waves in the plasma which can be used to optimize the magnetic geometry in toroidal systems. If successful, significant improvements in the stable plasma pressure limits and confinement in a steady-state plasma equilibria may be possible.

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[1] NRC Report on Plasma Science, National Academy Press, Washington, DC 1995.

Chaos, Turbulence, and Transport: One of the challenges in controlling an ignited fusion plasma is the dominance of the fusion heating source over any external power source in determining the plasma pressure profiles. Recent discoveries in the suppression of plasma turbulence in the core of a collisionless tokamak plasma open up the possibility of a significant improvement in confinement as well as a possible method of modifying the pressure profile in an ignited fusion plasma for improved stability in a steady-state equilibrium. (See Attachment 1 of Appendix D on Major Tokamak Facilities for additional detail.)

Plasma Sheaths and Boundary Layers: A critical unresolved problem in fusion energy is the development of techniques for the removal of heat and particles at the edge of the magnetically confined ignited fusion plasma. The leading candidate for this in a tokamak device is a divertor system which makes the transition from a very hot plasma at the plasma edge to a much denser low temperature plasma in contact with a material wall. (See Attachment 2 of Appendix D on Major Tokamak Facilities for additional detail.)

Magnetic Reconnection and Dynamos: A fundamental property of any magnetically confined plasma with finite conductivity is the possibility of magnetic reconnection which is a very important phenomenon in astrophysical, solar, and magnetospheric plasmas. Reconnection phenomena, usually described with a resistive-MHD model, are centrally involved in the dynamo activity seen in reversed field pinch configurations and in the sawtooth phenomenon seen in the core of tokamak plasmas.

B.3 Narrowing of the Support for Plasma Science

In the early 1980's, the fusion energy program in the United States was a relatively broad program which included a wide spectrum of experimental facilities and an array of alternative approaches to the tokamak. As the program positioned itself to move toward energy development with a short-pulse fusion ignition device in an environment of declining budgets, most of the program in alternatives was abandoned for budgetary reasons in the late 1980's. In response to Congressional policy direction in the early 1990's, the move to a schedule-driven energy development program has led to a further narrowing of the breadth of support for plasma science. This situation has led to considerable concern within the fusion and plasma science community and has been noted by both the PCAST review of magnetic fusion and the NRC Report on Plasma Science. Extracts from each of these reports are summarized below:

PCAST Report (page 36):

- although a strong core program has existed and continues to exist within the US effort, it is not as broad or as strong as is desirable, it is focusing increasingly on support of the ITER and TPX projects, and it is in danger of being squeezed down to inadequacy by the drain on the budget generated by these projects as they enter their construction phases;
- the effort on alternative concepts, which was practically eliminated in the series of project cancellations brought on by program cuts through the 1980's and into the early 1990's, is now wholly inadequate;

NRC Plasma Science Report (pages 23 and 89):

- Unfortunately, in the past, many opportunities for fundamental scientific exploration were missed, in some instances because of funding constraints and in others because of changing priorities within the fusion program.
- ...painful choices have often had to be made between upgrading larger facilities to operate in high-performance regimes and increasing the scope of scientific investigations in intermediate-scale devices.
- If the present trend toward large experiments continues without adequate attention paid to a broader base of experimental facilities, a dangerous gap will develop in our ability to address the wide range of questions important to fusion-relevant plasma physics.

The damage caused by this narrowing of support for plasma science can be viewed from two distinct perspectives. From an internal perspective, there is a weakening of the fusion program's capability to innovate and to produce the needed advances in fusion-relevant plasma science. From an external perspective, the fusion program has diminished its capability to make significant and enduring contributions to the advancement of plasma science of broad benefit to all the other important fields of application of plasma science.

B.4 Conclusions and Recommendations

Conclusion: *The narrowing of the breadth of support for plasma science by the fusion program is seriously weakening the capability of the fusion program to make significant and enduring contributions to the advancement of plasma science.*

Recommendation: *A key goal of the restructured fusion program is to expand support for fusion science with basic plasma science as a key element. To achieve this goal, the fusion program should explicitly assume the responsibility to advocate and act as a steward for basic plasma science.*

While other agencies of the federal government provide limited support in this area, the fusion program is the primary beneficiary of advances made in the field of plasma science. In turn, the many related fields and applications benefit from developments in plasma science from the fusion and other programs. The fusion program should take the lead by establishing a program to support basic plasma science, while continuing to work with other federal agencies to provide additional support for more fundamental plasma science research.

The expected benefits to the fusion program from taking on this new responsibility include aiding the development of fusion energy development through advances in fundamental understanding of the behavior of high temperature magnetized plasmas and an improved interaction with related disciplines in the scientific community.

Recommendation: *An effective program to broaden the basic plasma science activity supported by the fusion program would require building up to a support level of about 5% of present fusion funding.*

Some mechanisms the fusion program could use to implement this program in plasma science are:

- take the lead and work with other funding agencies to establish a "critical mass" plasma science research effort in a larger fraction of the nation's research universities;
- Incorporate plasma science in the structure of the program office in a manner that provides visibility and supports the unique character required to carry out the plasma science program mission;
- support cutting-edge theory and experiments that might contribute to the long term rather than only the short term development of fusion energy;
- seek proposals for fundamental plasma science experiments which can be performed on major tokamak facilities in analogy to outreach programs on the University of Rochester Omega laser fusion facility;
- establish a Plasma Science Young Investigator Award program to stimulate appointment and provide support for new university faculty researchers in plasma science modeled after the successful programs in the NSF;
- broaden the academic base in the field through outreach to institutions not currently supporting plasma science;
- support undergraduate programs in plasma science research and education.

While the need for establishing this program is immediate, success will require a sustained effort by the fusion program to establish the necessary research infrastructure in plasma science.

Adoption of these suggested mechanisms would also be a significant step by the federal government in implementing the recommendations of 1995 NRC Report on Plasma Science.

Appendix C

Theory and Computation

Theory and computation, in conjunction with experiment, provide the predictive capability at the core of the scientific research endeavor. Great progress in the understanding of plasma and fusion physics has been made in recent years (e.g., turbulence, stochasticity and chaos, magnetic reconnection, stability and wave-particle interactions), thanks to the interplay between theory and the other elements of the fusion research program.

Theory is cost effective and has high leverage. Quantitative predictions of plasma behavior are becoming increasingly accurate and contribute to the effectiveness of the experimental program. On the other hand, progress in the experimental program has produced measurements of more theory-relevant quantities (such as current density profiles and core turbulence, for instance), which in turn uniquely guide the theory effort towards a more realistic description of the plasma. This impetus should be maintained.

The United States remains a world leader in theory and computational modeling. For example, theory input would be valuable in foreign collaborations such as on ITER and stellarators, and in assessment of proposed innovative experiments. We are well positioned to contribute to and benefit from the worldwide fusion effort.

The theory program combines many areas of expertise -- from fundamental physics issues, to development of numerical algorithms to solve specific problems, to application of such tools in the context of a particular experimental need. All parts of the program benefit from interaction amongst all those areas, and with experiment. For instance, it seems clear that new parallel computers and gyrokinetic algorithms, combined with experimental benchmarking, will yield dramatic progress in our understanding of electrostatic turbulence within a few years. MHD tools are just now being usefully extended to cover resistive modes.

Even though the U.S. theory program has been effective and successful in the past, with its expanded scope and with an expected increase in computational activity the theory program will be hard pressed to meet its objectives even if budgets remain steady. In consequence, we point to some **recommendations** that would improve the present status:

1) Basic plasma physics: The restructured fusion program will support fundamental research in both plasma and fusion science, both experimentally and theoretically. This enhances cross-fertilization opportunities and scientific visibility, stimulating greater topical diversity in fusion research. It provides a better context for plasma physics as a legitimate science and an important

participant in the quest for knowledge. From the theory point of view, this renewed emphasis on scientific research poses a welcome challenge, which will strengthen the whole program internally and externally.

2) Evaluation: In a constrained budget environment, the efficient utilization of available resources is of paramount importance. A transparent, merit-based peer-review process should be in place to coordinate the theory and computation effort, choose and support the best performers, and terminate funding when undeserved. At the same time, a purely goal-oriented, project-driven management structure would be detrimental to the most innovative elements in the program. A balance needs to be maintained between short-term needs and the long-term pursuit of excellence.

3) Coordination: Applied theory and computation are usually team efforts. Increased coordination and flexibility in applied theory and computation, based on scientific goals, should be established at both the OFE level and amongst institutions. Where possible and appropriate an effort should be made to restructure the review process into consideration of units of topical working teams, rather than by institutions. There is clear benefit in pooling the available resources and reducing unnecessary multiplication of efforts towards the development of numerical tools. Utilization of numerical tools for experimental purposes may need to be funded in conjunction with experimental proposals.

4) Equipment: The rapidly evolving computational hardware and technology environment calls for flexibility in the assessment and purchasing of equipment. The fusion program needs to position itself to take advantage of new developments such as DOE's Advanced Scientific Computing Initiative (ASCI) project, which is expected to deliver 0.5 Teraflop performance this year. Increased computational speed, together with algorithm development, will contribute to essential predictive capabilities in plasma physics. Adequacy of computational resources needs to be assessed and provided as needed.

5) Education: On the educational level, the restructured program encourages a broad treatment of plasma science in undergraduate and graduate courses, including basic physics subjects such as low temperature and non-neutral plasmas, nonequilibrium statistical mechanics, nonlinear dynamics, aspects of astrophysics, etc. This intellectual diversity will help attract young talent into the field. At the same time, it will provide better postgraduate opportunities for young scientists. At the moment there is great demand in the semiconductor industry for knowledge in basic plasma physics, since the manufacturing process requires research on plasma-material interactions. The fusion program is best positioned to meet this educational demand, and theory efforts should consider these short-term needs as well as the longer-term pursuits of scientific excellence and fusion energy.

6) Renewal: Targeted support for valuable young scientists would help renew the human resources of the program. This is needed to help talented young scientists remain in the field after graduation, or enter it from other disciplines (in all types of institutions). A Plasma Science Young Investigator Award program should be considered. As in other areas of research, a long series of postdoctoral appointments is detrimental to the healthy renewal of the field. Provisions should be made for the availability of less evanescent employment.

7) Communication: The Sherwood Theory Meeting could contribute to the broadening of the program by explicitly welcoming research in all areas of plasma physics, and seeking participants from relevant related fields of physics. Joint theory-experiment ventures (such as the Transport Task Force, the Divertor Theory Task Force, or the MHD working group) facilitate the internal interaction of physicists in the program, by being specifically goal-oriented. They should be encouraged. At the same time, other possible avenues for communication (besides meetings) should continue to be explored: exchange of researchers, visiting positions, electronic-mail bulletin boards, dissemination of drafts and preprints, publications, etc. These other forms of communication are less constraining and allow for greater diversity than purely topical efforts.

Appendix D

Major Tokamak Facilities

This appendix concerns the program's three major tokamak research facilities DIII-D, Alcator C-Mod, and the Tokamak Fusion Test Reactor (TFTR). Together they comprise the largest element of the U.S. magnetic fusion program (about 40% of the budget). These are world-class facilities with outstanding records of scientific accomplishment. Their scientific programs contribute in a major way to U.S. goals and contribute stimulating ideas and results to the world fusion program. They are centers of collaboration for both U.S. and foreign scientists, providing opportunities to test new ideas and to advance fusion science. They provide a basis for international collaborations such as scientific personnel exchanges, joint experiments, and contributions to the physics basis for the International Thermonuclear Experimental Reactor (ITER). In this appendix we provide an assessment of the major facilities' scientific capabilities, expected contributions to program goals, and their future in the restructured fusion program.

D.1 Context: U.S. Program Strategy

In considering the tokamak facilities, the priorities for the restructured fusion science program are: 1) basic science of high temperature magnetically confined plasma; 2) confinement concept improvement; 3) burning plasma physics; and 4) low-activation materials. This set of priorities is consistent with those recommended by the President's Committee of Advisors on Science and Technology (PCAST) [1] and those reflected in the Office of Fusion Energy's draft strategy document [2]. The major tokamak facilities must clearly be considered in light of this strategy:

Basic Plasma Science. Underlying progress towards fusion is the development of a basic understanding of the behavior of magnetically confined plasmas. Fusion program support for plasma science provides the primary two-way interface between the field of fusion energy and the scientific community. The highly collisionless plasmas created in the major tokamak facilities are a unique plasma environment and have served to drive important advances in areas such as magnetohydrodynamic (MHD) stability, plasma turbulence and transport, wave-particle interactions, and atomic physics. In the restructured fusion program seeking to reinvigorate its support for basic plasma science, the major tokamak facilities will play an important role as national centers hosting experiments directed at advancing our basic scientific understanding.

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[1] "The U.S. Program of Fusion Energy Research and Development," Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995, p. 2.

[2] "Strategy for a Restructured U.S. Fusion Energy Research Program," U.S. Department of Energy, Office of Fusion Energy, draft of 11/29/95

Confinement Concept Optimization. This is the focus of "a strong domestic core program in plasma science and fusion technology, with funds to explore both advanced tokamak research and research on concepts alternative to the tokamak, leveraged where possible on related activities worldwide." (See PCAST report.) The OFE strategy calls for "research to enhance the performance and attractiveness of tokamaks, conducted primarily through reliance on existing facilities." [3] Thus, concept improvement, with a primary reliance on U.S. domestic resources in the near term, is a key goal for the major tokamak research.

Burning-Plasma Physics. Both the PCAST and the OFE strategy recognize that an understanding of the physics of burning plasmas is a critical goal for fusion. The major U.S. tokamak facilities have an important role in contributing to a sound physics basis for burning plasma experiments. Essential contributions to the physics basis for ITER are needed from the U.S. tokamaks, and they represent commitments to critical ITER Engineering Design Activity (EDA) deliverables. For the exploration of long-pulse, strongly self-heated plasmas, the U.S. strategy is to use international collaboration.

D.2 Scientific Issues for Tokamak Concept Improvement

There has been enormous improvement in plasma confinement and stability over the last 20 years. This has been the direct result of our increased understanding of plasmas from our experimental and theoretical investigations. The developed theory and modeling indicates that further substantial improvements are possible, and are supported by recent encouraging experimental results. The "improvements" sought will reduce the construction and operating cost of tokamak-based fusion power plants:

- Make tokamaks steady state. To eliminate cost of energy storage systems needed to smooth output, and to reduce cost of extra structure needed to handle cyclic loads.
- Reduce the frequency and severity of disruptions. To reduce cost of service interruption and recovery.
- Make tokamak systems more compact. To reduce cost of the reactor core and associated hardware.

Table 2.1 summarizes the scientific issues and key capabilities associated with realizing these improvements. Some of these are discussed in greater detail in attachments on Transport Barrier Issues, on Power and Particle Exhaust, and on Profile Control and Auxiliary Heating Physics and Technology.

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[3] The other "prong" of the OFE strategy, "expansion of the exploration of promising alternate plasma confinement approaches," is the subject of another appendix and is not addressed in this appendix except insofar as it relates to possible replacements for the three major tokamaks.

Table 2.1. Scientific Issues for Tokamak Improvement

Scientific Issues	Tokamak Improvement Benefit	Key Capabilities
High beta stability	High self-generated current (steady state) High fusion power density (compactness)	Plasma heating and current-drive systems, plasma shape control.
Transport control	Reduce plasma current and recirculating power requirements; compactness; control of self-generated currents	Plasma heating and current-drive systems: neutral beams and RF; shear flow and rotation control; particle reflux & boundary control.
Non-inductive current drive, including self-generated currents	Plasma sustainment (steady-state) and profile modification to allow operation near stability limits (reduced disruptivity)	Plasma heating current-drive systems.
Power and particle exhaust, boundary physics	Compatibility between the first wall/pump and a high-performance plasma (steady state, compactness, reduced disruptivity); increased fuel density	Fueling systems; wall-conditioning techniques; helium pumping techniques; high heat-flux targets.
Integration of improvements	Optimum performance and high availability	Control systems.

The keys to tokamak improvement are plasma control, and power and particle exhaust. The exciting advances in plasma performance that have been achieved in recent years have come about through control of the plasma shape, plasma profiles, plasma transport, and boundary conditions. Recent progress in the development of methods for exhausting the heat and particles out of tokamaks that are compatible both with clean, high-performance plasmas upstream and practical target structures downstream is extremely encouraging. In both of these areas, outstanding diagnostics, analytical tools, and theory have allowed us to understand the cause-and-effect relationships well enough to design improved techniques that can be implemented and tested on the available facilities at low cost.

To be successful, the improvements discussed must ultimately be achievable simultaneously in a self-heated (burning) plasma. The enabling control and diagnostic technologies must be developed to be efficient and lead to economical reactor solutions. Well-integrated tokamak design projects are needed to determine realistic engineering constraints and guide the scientific research toward practical solutions. Simply stated, the aim of tokamak concept improvement research is to develop plasma control and exhaust strategies to improve the performance and reliability of the tokamak, and to test them in a burning-plasma experiment.

Conclusion: *The aim of tokamak concept improvement research is to gain the predictive capability needed to develop plasma control and exhaust strategies that will improve the performance and reliability of the tokamak and, ultimately, to test them in a burning-plasma experiment.*

D.3 Scientific Issues for Burning Plasma Physics

The motivation for studying the physics of burning plasmas is compelling: an economical fusion power reactor must be primarily self-heated. The power injected from external systems to control the plasma and to offset its energy losses cannot be more than a few per cent of the fusion power output. Self-heating involves the deposition of the energy from alpha particles produced in fusion reactions into the plasma. As PCAST noted (p. 21-22), "the alpha particles can influence the plasma behavior in ways that are difficult to predict," and therefore the issues must be studied experimentally in order to gain the predictive capability needed to design fusion power plants. While some progress on these issues can be made in existing experiments, it is clear that a burning-plasma experiment will be needed to resolve them completely. Table 3.1 summarizes the scientific issues associated with attaining this predictive capability.

Conclusion: *The aim of burning-plasma physics research is to gain the predictive capability on the behavior of burning plasmas needed to design fusion power plants. While some progress on these issues can be made in existing experiments, a burning-plasma experiment will be needed to resolve them completely.*

Table 3.1. Scientific Issues for Burning Plasma Physics

Key Feature	**Scientific Issues**
Fusion Alpha-particles	• Identify and minimize the impact of alpha-driven instabilities on plasma performance. • Understand alpha-particle thermalization and heating of the core plasma. • Interaction of alpha-particles with RF waves (for heating and current-drive) • Effect on beta limits and particle transport.
DT Isotope Effects	• Effect on energy and particle transport and transition boundaries between different plasma confinement modes. • Compatibility of external heating and current drive techniques with DT fuel.
Helium transport and exhaust	• Scaling of helium ash transport out of the core and in the scrapeoff / divertor plasma.
Test of advanced plasma control and exhaust strategies in burning plasmas.	• Assess prospects for realizing tokamak concept improvement benefits in reactors. • Determine beta-limits consistent with long pulse lengths and self-heating. • Develop models for the scrape-off and divertor plasma performance to ensure compatibility between the first wall/pump and a burning plasma. • Develop models for plasma fueling.

The U.S. burning plasma physics strategy is to collaborate in an international burning-plasma experiment, for which ITER is the current vision. In order to design a burning-plasma experiment (whether ITER or another embodiment) and to support a decision on its construction, it is necessary to first establish a sufficient physics basis to permit an assessment of the facility's potential capability to accomplish its mission. That physics basis is currently incomplete. For that reason, the ITER EDA includes a physics research and development (R&D) program, involving all the parties, whose purpose is to complete the necessary physics basis for ITER.[4] The current scientific issues for this program are summarized in Table 3.2. This table also indicates the U.S. tokamaks expected to contribute toward each issue.

As part of the U.S. burning-plasma physics program, the major tokamaks contribute to the resolution of ITER Physics R&D issues. The U.S. has maintained a reputation for innovation and excellence in its contributions to the scientific basis for ITER since the ITER program's inception in 1988. This is because developing good scientific models, comparing to well diagnosed experiments, and then extrapolating to burning plasma conditions is a major strength of the US fusion program. Without these critical contributions, the ITER physics basis will be weakened, and it will likely become necessary for our international partners to re-plan their programs to provide the needed data. This is an important consideration in the event that one or more of the facilities must be closed.

Conclusion: *The major U.S. tokamaks make critical contributions to the physics basis for the International Thermonuclear Experimental Reactor (ITER), currently planned as the next-step burning-plasma physics experiment.*

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[4] For planning purposes, it should be noted that ITER physics R&D is "voluntary," meaning that it is funded out of the fusion programs but not the ITER budgets. All three U.S. tokamaks devote part of their program to addressing ITER R&D needs.

Table 3.2. Scientific Issues for Constructing a Burning Plasma Facility: Current ITER Physics Research and Development Needs

Scientific Issues	Information required from tokamak facilities and primary U.S. contributors	Benefit to Burning Plasma Facility Assessment Capability
Power and particle handling	• Demonstration of critical performance attributes, e.g., peak heat load reduction via radiation, He pumping. (DIII-D, C-Mod) • Database on scrape-off and divertor plasma conditions over a range of parameters and machines. (DIII-D, C-Mod)	• Reduce uncertainty in predicting divertor performance. • Reduce uncertainty in predicting first-wall erosion rates.
Disruptions	• Data on the characteristics of disruptions under conditions prototypical of ITER over a range of parameters and machines. (C-Mod, DIII-D, TFTR)	• Determine electromagnetic loads for design of in-vessel structures. • Reduce uncertainty in predicting first-wall erosion rates.
Confinement	• Database from demonstration discharges prototypical of ITER over a range of parameters and machines. (DIII-D, C-Mod) • Data on confinement in DT plasmas. (TFTR)	• Reduce uncertainty in confinement projections and confinement-mode transition boundaries. • Assess prospects for exceeding standard density limits.
Beta limits	• Data on the role of non-ideal magnetohydrodynamic effects. (DIII-D, TFTR, C-Mod)	• Reduce uncertainty in predictions of beta limit and fusion power output.
Alpha-particle effects	• Data on instabilities and losses from DT experiments or beam simulations. (TFTR, DIII-D)	• Reduce uncertainty in predicting the impact of alpha effects on performance.
Advanced plasma modes (with Improved-Tokamak characteristics)	• Data on operating modes with enhanced performance compared to ITER physics model. (DIII-D, C-Mod, TFTR)	• Assess benefits to mission of expanded operational flexibility. • Determine hardware upgrade requirements to provide plasma controls for advanced modes.

D.4 Machine Capabilities for Tokamak Improvement Research

Our tokamak-improvement research to date has given us a good understanding of the machine capabilities that are important for optimum performance and for making further progress toward the goal. These are described in Table 4.1. Further research is needed to fully define the requirements for control of the plasma shape and of the current, pressure, density, and rotation profiles. We know these will involve the use of plasma heating and current-drive techniques (neutral beams and radiofrequency waves) and fueling techniques, but we do not yet know the best combination and configuration of such systems. In addition, power and particle control techniques and flow-shear control techniques consistent with these advanced operating regimes must be evaluated. These are important areas for innovative research and development both in the plasma physics and in the enabling technologies.

Table 4.1. Tokamak Machine Capabilities Important for Tokamak Improvement Research.

Capability	Importance
Cross section shaping flexibility	–Maximizes β. –Minimizes major radius at fixed ignition margin. –Controls transport and stability.
Poloidal divertor	–Tests advanced-mode compatibility with reactor-relevant power and particle exhaust system. –Control of edge profiles. –Control access to high-performance regimes. –Control of particle sources (hydrogen isotopes, helium, impurities).
Reactor-like core or divertor plasma parameters	–Reactor relevant beta and collisionality (transport). –Reactor relevant non-ideal effects on magnetohydrodynamics. –Reactor relevant atomic physics (power handling).
Deuterium-tritium plasma	–Reactor-relevant fuel (affects confinement and wave-heating physics). –Tests advanced-mode compatibility with α-particles, He exhaust, and α heating (if reactivity is high enough). –Tests of alpha-heating control (e.g., α-channeling).
Current profile control.	–Increases β-limit beyond standard scaling. –Compensate for misalignments in bootstrap current profile. –Control access to high-performance regimes.
Heat and fuel deposition control	–Control transport and pressure, density profiles –Optimize pressure profile for stability, bootstrap alignment, reactivity. –Simulate α-heating profile in experiments.
Rotational shear control	–Controls transport and stability via radial electric field.
Close-fitting conducting wall and rotation control	–Controls stability limits of high-β, high-bootstrap modes.
Diagnostics (spatially and temporally resolved measurements)	–Profile and fluctuation diagnostics for understanding of plasma-control effects. –Boundary and divertor diagnostics for understanding of exhaust mechanisms. –Alpha diagnostics for understanding α dynamics in advanced regimes. –Real-time signals for active feedback control. –Essential for developing predictive capability through theory and modeling.
Long pulse length	–Tests advanced-mode sustainment beyond current-relaxation time (if greater than τ_{skin}). –Tests advanced-mode sustainment beyond plasma-wall equilibration time (if greater than 100's of seconds). –Tests plasma reliability against disruptions (if many hours).

The tokamak-improvement capabilities of the three major U.S. tokamak facilities are summarized in Table 4.2.

While none of the three U.S. major tokamak facilities is fully prototypical of an advanced fusion reactor, together they provide a strong collection of capabilities for tokamak improvement studies as well as strong scientific teams. DIII-D's strong shaping and shape flexibility, pumped divertor, and high beta values give it the important advantage of a plasma configuration that most closely resembles that expected in an attractive tokamak reactor. TFTR's important advantage is its capability for deuterium-tritium operation and potential capability for significant alpha

Table 4.2. Capabilities of Major U.S. Tokamaks for Tokamak Improvement Research (future capabilities in parentheses)

Capability	DIII-D	Alcator C-Mod	TFTR
Plasma shape*	Strong D-shape: κ=0.9-2.6, δ=-0.1–1.0	D-shape: κ=0.9-1.85, δ=0-0.8	Circular: κ=0.8-1.2
Boundary	Single or double null poloidal divertor, inner-wall limiter	Single or double null poloidal divertor, inner-wall limiter	Inner wall limiter, inboard X-point.
Deuterium-tritium fuel			yes
Particle control	neutral beam injection, pellets, pumped divertor, pumping carbon wall	pellets, metal wall, (pumped divertor)	neutral beam injection, pellets, pumping carbon wall
Current profile control.	fast-wave, electron cyclotron, neutral beam, (mode-conversion)	fast-wave, mode-conversion, (lower hybrid)	fast-wave, mode-conversion, neutral beam, (lower hybrid)
Heating profile control	neutral beam, ion-cyclotron, electron-cyclotron, fast-wave, (mode-conversion)	ion-cyclotron, mode-conversion, fast-wave	neutral beam, ion-cyclotron, mode-conversion, fast-wave, (ion Bernstein waves)
Rotation-shear control	controlled L-H transition, co- neutral beam/non-axisymmetric field perturbation, RF-NBI interaction.	controlled L-H transition	co-, ctr-, or balanced neutral beam, RF-NBI interaction, (ion Bernstein waves)
Conducting wall stabilization	yes		yes
Diagnostic strengths	profile, fluctuations, and divertor	divertor and disruptions	profile, fluctuations, and alphas
Long pulse length	10 s @ B=2.1 T, 20 s @ B=1.7 T	1 s @ B=9 T 10 s @ B=4 T	2 s @ B=6 T 10 s @ B=4.4 T

*Shape parameters: elongation, κ = (plasma height)/(plasma width); triangularity δ : 0=elliptical, 1=dee.

heating. Its high field and larger size result in its having a normalized gyroradius closer to that expected for tokamak fusion reactors. Alcator C-Mod is presently configured to study divertor physics, confinement, and disruptions. Its high magnetic field strength and compact size put it in a unique parameter regime and provide useful "scaling leverage" through its contributions to world databases. Profile control systems in preparation would enhance its tokamak-improvement capabilities in the future.

The capabilities of the major foreign tokamak facilities are presented in Table 4.3. The capabilities and program plans for these machines are considered in the assessment of the U.S. plans in Section 5. Clearly significant potential for tokamak improvement research exists outside the United States. Tokamak data analysis increasingly relies on multi-machine databases which are used to test predictive models and determine scalings. Observations of performance advances in one facility require confirmation in others to build confidence in their reliability, as is usual in science. The existence of multiple tokamaks worldwide with overlapping but distinct capabilities and programmatic foci is therefore important for making steady progress toward scientific goals.

Conclusion: *The major U.S. tokamaks provide a strong collection of capabilities for tokamak improvement studies as well as strong scientific teams. The U.S. tokamaks have led this area of research and contributed greatly to its progress.*

Table 4.3. Capabilities of Major Foreign Tokamaks for Tokamak Improvement Research. (future capabilities in parentheses)

Capability	**JT-60U**	**JET**	**Asdex/U**	**Tore Supra**
Plasma shape	D-shaped	Elongated	Elongated	Circular
Boundary	Single null poloidal divertor	Single null poloidal divertor	Single null poloidal divertor; metal coating	Inner wall and local limiters; ergodic magnetic limiter
Pumping and walls	pumping carbon wall, (pumped divertor)	pumped divertor, pumping carbon and beryllium wall	pumped divertor, pumping carbon wall	pumped limiter, pumping carbon wall
Deuterium-tritium fuel		yes		
Current profile control.	lower hybrid, fast-wave, neutral-beam	fast-wave, lower hybrid, neutral beam, mode-conversion	neutral beam, fast-wave, mode-conversion, (electron-cyclotron)	fast-wave, lower hybrid, mode-conversion, neutral beam, (electron-cyclotron)
Rotation and rotation-shear control	co- and perp- NBI	co- NBI	co-NBI	
Long pulse length	20 s	20 s	5 s	60-(1,000) s

D.5 Program Plans

The vision of tokamak improvement presented here is the same one that set the goals for the formerly planned Tokamak Physics Experiment (TPX). Stimulated by this vision, the plans for the major U.S. tokamaks have been formulated in recent years to include a strong emphasis on tokamak improvement (often denoted "advanced-tokamak, " or "AT") issues.

DIII-D. The DIII-D program focuses on advanced-tokamak operating modes, divertor physics, and core transport physics in a shaped, poloidally-diverted configuration. An upgrade program including on- and off-axis current-drive systems and a flexible divertor structure is in the process of being implemented to extend their capabilities for tokamak-improvement research. The strong coupling among plasma stability, transport, and power and particle exhaust is highly nonlinear in a strongly shaped plasma, and DIII-D is unique in its ability to evaluate this complex relationship. Integration of an advanced high-beta core plasma with a pumped radiative divertor for 5-20 s pulses is a key goal. Also, DIII-D has been a strong contributor to the physics basis for ITER, especially in areas of divertor physics, disruption physics, confinement scaling and data bases, beta limits, and helium transport. A number of foreign tokamaks have capabilities similar to DIII-D's: shaping (JET, JT-60U), current-drive (JET, JT-60U, Tore Supra), and divertors (JET, Asdex-U, JT-60U). However, DIII-D is unique because of its shaping flexibility and pumped double-null divertor with slot-length variability, in combination with advanced-tokamak profile controls and diagnostics.

Alcator C-Mod. Designed with high magnetic field capability, Alcator C-Mod provides physics data and understanding on numerous critical issues, including enhanced core confinement, dissipative divertor studies, and detailed disruption investigations. The high field, compact approach results in a flexible research facility which accesses unique regions of dimensional parameter space. Because of its closed, all metal vertical-plate divertor geometry, combined with unique reactor level parallel power flows and excellent divertor diagnostics, Alcator C-Mod explores the dissipative divertor physics which will be important for any reactor, including the present ITER design. Long pulse capability (at somewhat reduced field), combined with planned current drive upgrades, will allow investigation of advanced tokamak plasma regimes for up to 10 skin times. The Alcator program is integrated into the academic environment at MIT, with more graduate students than Ph.D. scientists on the team, and as such, it is cost effective and well suited for training members of the next generation of fusion plasma scientists.

Tokamak Fusion Test Reactor (TFTR). In FY-1995, a proposal for a three-year extension of TFTR was put forward. Its aim is to utilize improved performance regimes and profile control to increase fusion power output and extend studies of alpha-particle dynamics and their influence on the main plasma. The program extension includes upgrades to the radiofrequency heating and current drive systems to develop and test profile-control tools in a deuterium-tritium plasma. Significant alpha heating is predicted, including the possibility of thermal runaway in the center of the discharge. The TFTR program is investigating techniques for controlling the core plasma transport in reacting plasmas, and for demonstrating "alpha channeling," a novel concept for profile control and reactivity enhancement. Important areas where TFTR contributes to the ITER physics data base include isotope effects on transport and radiofrequency heating techniques, helium transport, MHD stability limits, and operation in a deuterium-tritium, high-neutron-flux environment. In the world program, only the Joint European Tokamak (JET) also has the capability to perform deuterium-tritium experiments. However, TFTR is conducting a more extensive deuterium-tritium physics campaign through the end of the ITER EDA, and has unique diagnostics for measuring the confined alpha population and alpha particle losses to the boundary. It is uniquely pursuing the study of alpha heating control techniques. In addition, TFTR has more extensive diagnostics and pressure-profile control capabilities for studying improved-performance regimes.

In Table 5.1, we summarize the key contributions expected from the three machines in the next few years.

Conclusion: *All three major tokamaks have program plans which are consistent with the goals of the restructured U.S. fusion science program. Each is well positioned to make further scientific advances.*

Table 5.1. Expected Near-Term Contributions to U.S. Program Goals from the Three Major Tokamaks

Strategic element:	**Tokamak Improvement**	**Burning-Plasma Facility (ITER) Physics Basis Support**
DIII-D Contributions:	• Develop self-consistent high-performance advanced-tokamak (AT) scenarios suitable for steady state - high beta - high confinement - high bootstrap fraction - current drive - power and particle control • Demonstrate long pulse AT (10 -20 sec) scenarios. • Non-inductive current drive development/applications (ECCD, FWCD, MCCD, bootstrap). • Evaluate dependence of stability on shape, current profile, pressure profile, rotation profile, stabilizing effect of a resistive wall. • Develop understanding of enhanced confinement in AT scenarios, effects of sheared rotation, current profile, and shape. • Demonstrate density control with divertor pumping and pellets. Demonstrate real-time feedback control of the profiles.	• Confinement scaling with effective size (a/ρ) in reactor relevant plasma. • Develop physics understanding and scaling of L-H and H-L transitions, with detailed edge diagnostics. • Validate disruptions models with detailed diagnostic measurements; demonstrate avoidance and mitigation. • Understand beta limit in shaped discharges with fully-penetrated profiles. • Develop physics understanding and scaling of ELMs. • Develop and validate divertor models with detailed edge diagnostics. • Understanding of different divertor geometries, single and double null, variable slot. • Demonstrate density beyond Greenwald limit in high-performance ELMing H-mode. • Helium transport and exhaust with core source. • Demonstration of advanced tokamak modes compatible with ITER design • ρ^* scaling in strongly shaped plasma to demonstrate compact ignition scenario at high beta. • NBI driven Alfvén eigenmodes in shaped discharges, dimensionally similar to ITER.

continued...

Table 5.1. Expected Near-Term Contributions to U.S. Program Goals from the Three Major Tokamaks, continued

Strategic element:	**Tokamak Improvement**	**Burning-Plasma Facility (ITER) Physics Basis Support**
Alcator C-Mod Contributions:	• Demonstrate/understand radiative and detached divertor operation consistent with enhanced confinement modes (H-Mode, PEP) - closed vertical plate pumped divertor - impurity screening, impurity transport • High-Z first wall with low-Z coatings - impurity radiation, transport - wall conditioning physics for metal walls • ELM physics - type III, grassy • Enhanced confinement physics - high density, field, current density - effects of shape • ICRF physics - mode conversion - fast wave at high density - fast wave current drive • Understand disruption dynamics (halo currents) • Evolution of AT profiles (FY 2000) - ~10 skin time capability	• Investigation of dissipative pumped divertor - ITER/reactor dimensional parameters (density, temperature, power flow) - reactor-like dimensionless parameters (collisionality) • Disruption dynamics - halo currents (detailed diagnostics) - shaping, high current density - amelioration (killer pellets) • Metal first wall operation - divertor dynamics, atomic physics - plasma wall interactions (erosion, sputtering, redeposition) • Study of techniques for low Z coating of metal walls (ECDC boronization, low Z pellets) • Investigation of B-field compatible wall conditioning (ECDC) • Density limit physics (H and L Mode) - scaling with current density - compare gas with steady state pellet fueling • Core and edge confinement - size, field and density effects - barrier thresholds - ELM physics • ρ* scaling (with DIII-D and JET)

continued...

Table 5.1. Expected Near-Term Contributions to U.S. Program Goals from the Three Major Tokamaks, continued

Strategic element:	**Tokamak Improvement**	**Burning-Plasma Facility (ITER) Physics Basis Support**
TFTR Contributions:	• Develop/evaluate reactor relevant pressure profile controls: - sheared rotation control (IBW) - alpha heating profile modification - alpha channeling • Documentation of transport in advanced-tokamak (AT) configurations - role of q-profile - role of electric fields/rotation - particle transport • Evaluate alpha-Alfvénic and MHD stability with AT profiles - low central shear with q(0) > 1 - strongly reversed central shear • Initial effect of self-heating on pressure profile control • Evaluate current drive techniques for AT scenarios - MCCD, LHCD - bootstrap with strong flow shear • Active fueling of AT configurations (pellets and NBI) • Low-Z first wall coatings	• Beta limit dependence on non-ideal effects • Disruption studies: - runaway electron production - killer pellets - integrated modeling • Tritium isotopic effects on confinement and ELMs • Systematic validation of predictive transport models • Helium transport with core sources. • Effect of sawteeth and MHD activity on alpha profiles and loss • Effect of alphas on MHD and Alfvénic stability • Alpha heating • ICRF heating and current-drive in DT plasmas - fundamental deuterium / second-harmonic tritium - minority tritium current drive - off-axis MCCD in DT plasmas - high-density effects - antenna optimization - advanced high power-density launchers • Density limits with enhanced confinement.
Goal:	Provide the physics basis for a attractive-tokamak control and exhaust strategy for a burning-plasma experiment. (long-term)	In collaboration with the partners, provide a sound physics basis for ITER construction.

In FY-1996, all three tokamaks are operating on severely limited schedules with reduced staff and little upgrade activity because of severe budget reductions. This is highly undesirable as a long-term solution. As a matter of good practice, any operating experiment should be supported with healthy funding to operate cost-effectively. It must have the resources (operating time, hardware upgrades, and scientific staff in appropriate balance) to be fully productive.

Conclusion: *All three major tokamak facilities are currently operating in a low-productivity manner because they are under-funded.*

Recommendation: *As a matter of good practice, any operating experiment should be supported with healthy funding to operate cost-effectively. It should have the resources (operating time, hardware upgrades, and scientific staff in appropriate balance) to be fully productive.*

D.6 Near-Term Program

Restructuring the fusion program within budget levels that are greatly reduced from previous years may make it necessary, in the near term, to retire one of the major tokamak facilities. The program of deuterium-tritium plasma studies currently under way in the Tokamak Fusion Test Reactor (TFTR) can be completed in the relatively near-term, whereas the programs on DIII-D and Alcator C-Mod extend for a longer term. It is appropriate, then, that TFTR should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from this facility.

Recommendation: *The Tokamak Fusion Test Reactor (TFTR) should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from it, since its program of deuterium-tritium plasma studies can be completed in the near-term relative to the other major facilities.*

The combination of constrained budgets with rapid shifts in program directions may necessitate a premature termination (i.e., within a period significantly less than two years) of the TFTR program. If this is required, some or all (depending on the time remaining) of the following research objectives would not be completed:

- Plasma self-heating with core alpha-particle heating locally comparable to external heating.
- Control of plasma transport and stability using ion-Bernstein-wave generation of sheared flow.
- Control of alpha-particle thermalization and heating profiles using externally driven waves.
- Radiofrequency heating and current drive in deuterium-tritium plasmas (e.g., for ITER).

It is unclear when these lost scientific opportunities would return. For this reason, we believe that sufficient resources should be provided to operate TFTR at high productivity throughout FY-1997, while also operating DIII-D and Alcator C-Mod at high productivity. In any event, the DIII-D and Alcator C-Mod programs should be supported for full productivity after TFTR is retired during FY-1998. If this is done, the progress that can be expected, based on current planning, is indicated by the contributions listed in Table 5.1. Moreover, it is anticipated that the scientific output of these facilities will broaden in a restructured program that is less tightly focused on energy development. Research results will lead in new directions and we can expect many scientific contributions in addition to those listed in the table. To plan effectively, we believe that there must be a national review (with international participation) of the program for the three facilities. This review should be used to identify the highest-priority objectives to be accomplished on TFTR prior to its termination, and to prioritize and coordinate the efforts on all three facilities in the context of the restructured national program.

Recommendation: *A national review (with international participation) of the program for the major facilities should be conducted to identify the highest-priority objectives to be accomplished on TFTR prior to its termination, and to effectively prioritize and coordinate the efforts on all three facilities in the context of the restructured national program.*

D.7 Implications for Future Facilities

For the next several years, the U.S. fusion program will be limited to two major tokamak facilities (after the completion of the TFTR program). After about 2001, it is likely that DIII-D will have completed its useful life and a new facility will be needed to maintain a strong program. Based on present knowledge, one could envision possible goals for successor facilities to extend tokamak-improvement research, for example:

- Integrating attractive core and divertor operating scenarios for long pulse lengths.
- Further developing the physics basis for extreme compactness.
- Integrating advanced-tokamak scenarios in a deuterium-tritium environment.

Indeed, a number of interesting configurations have been suggested. We expect, however, that our visions will evolve over the next few years based on new developments, not only from the major U.S. and foreign tokamaks, but also from smaller experiments and new ideas stimulated by the re-orientation of the U.S. program. For this reason, we believe that the range of options for future major facilities should be expanded greatly. Innovative physics and engineering concepts, including non-tokamak designs, should be encouraged and considered. Assessments of the key physics and engineering characteristics of a wide range of options for future major facilities should be undertaken.

Recommendation: *Assessments of the key physics and engineering characteristics of a wide range of options for future major facilities should be undertaken.*

A burning-plasma experiment is a logical follow-on to the experimental program being carried out in the present generation of tokamaks. This can best be accomplished through international collaboration. The goals for such an experiment include not only the demonstration of controlled ignition and extended burn of a DT plasma, but also driven, high-beta, high bootstrap-current-fraction steady-state burn. This latter goal represents a continuation of the tokamak improvement program, and would be a logical niche for U.S. participation, even as a minor partner. At present, the principal international collaboration aimed at a burning plasma experiment is ITER. We believe that the goals of ITER are compatible with the aims of the U.S. fusion science program. To the extent possible, we should structure our program in such a way as to maintain the opportunity to be a credible participant in the scientific program of ITER.

Recommendation: *To the extent possible, the United States fusion science program should be structured in such a way as to maintain the opportunity to be a credible participant in the scientific program of an international burning plasma experiment.*

D.8 Attachment 1 to Major Tokamak Facilities Appendix
Transport Barrier Issues

Basic Goals

The goals of a research program aimed at developing an attractive steady-state tokamak clearly include pressure profile control and current profile control. These are non-orthogonal in the sense that pressure profile control may be achieved through control of the current profile, control of radial electric field shear via toroidal and poloidal rotation control, and control of particle sources and sinks. The tokamak performance benefits that are expected include: good energy confinement, high beta (for high power density), minimal current-drive power requirements, low (vanishing) disruptivity, and, most likely, high density (relative to present empirical limits), and compact size. It is understood that ultimately all of these benefits must be achieved self-consistently and simultaneously. This is the real challenge.

As pressure profile control requires understanding the space-time response function of the pressure profile, transport barrier dynamics is the key scientific concern at present. A "transport barrier" exists when the cross-field transport is reduced to near-neoclassical levels over some portion of the plasma. The dynamics of interest include:

a) threshold conditions for barrier formation, determined by local gradients in profiles, electric field, magnetic field structure, and shaping.
b) barrier propagation speed and profile steepening rate.
c) barrier location, determined by heat and fuel deposition profiles, momentum deposition profile, magnetic field structure, and shaping.
d) barrier limits, and mechanisms for their relaxation and termination [of which edge-localized modes (ELMs), x-events, and disruptions are examples].

Most improved-performance operating modes that have been observed in tokamaks exhibit transport barriers. These include:

a) "ELM-y H-modes," which have an edge barrier, obtained especially with peaked current profiles. Also external regulation techniques (e.g., shaping control, flow-shear control, etc.) are evolving.
b) "Enhanced reversed-shear (ERS)" or "negative central shear (NCS)," which have internal barriers.
c) "CH-mode" (and others) which have internal barriers facilitated by momentum deposition control with ion Bernstein waves (in PBX-M) or neutral beam injection (in JT-60U).
d) "VH-mode," with an edge barrier extending inward.
e) "Pellet-enhanced performance (PEP) mode," with an internal barrier driven by fuel deposition.

Our interest in barrier dynamics is driven by the recognition that a "steady state" tokamak will not in fact be static but will necessarily undergo control fluctuations, i.e., barriers will have to be lowered from time to time to facilitate particle removal or avoid stability limits. It is therefore critical to understand how to escape from barrier regimes to maintain high-performance operation. Specific scientific issues include:

1) H-mode to L-mode power threshold requirements.
2) Reversed-shear (either ERS or NCS) mode to L-mode transition hysteresis. Here, the nature of the back-transition is important for particle control (does the reversed-shear profile peel back like an onion, allowing artificial ELMs, or just collapse?)

One can expect that the various high-performance operating modes, each exploiting an optimized type of barrier mode, will evolve with time.

Research Leverage on Transport Barriers

A high-performance steady-state tokamak will need control of both pressure and current profiles to optimize transport within stability limits. As we have discussed, transport barriers will need to be controlled for regulation purposes. Means of barrier control include:

1) Magnetic field structure [q(r)] control via current-profile control.
2) Magnetic geometry control via active plasma shape control.
3) Electron and ion heat deposition profile control via heating-system configuration.
4) Particle deposition and removal control via pellets, neutral beams, wall conditioning, and divertor recycling control.
5) Momentum shear control via radiofrequency (e.g., ion Bernstein waves) torque shear and deposition control.
6) Boundary control via divertor, electrostatic biasing, pumping, and controlled L-to-H mode transition.

The flexibility to vary plasma parameter regimes also represents a means of control. Specifically, in addition to the well known critical dimensionless parameters such as beta, collisionality, and reduced ion gyro-radius, several others need to be identified, including:

1) The local parameters (density gradient?) underpinning the empirical (Greenwald) limit on density. The well-known result that shallow pellet injection triggers a slow relaxation suggests that disruption is not a fundamental problem. A key question is whether or not the Greenwald limit constrains the performance of reversed-shear modes.
2) The local parameters that determine barrier transition hysteresis, and relaxation below transition thresholds.
3) The local parameters that define regimes of tolerable relaxation compatible with heat and particle exhaust capacity. In H-mode, we prefer "grassy" ELMs to "giant" ELMs. In particular, the threshold of access to a tolerable ELMy H-mode regime is a central issue for edge barriers. Disruption avoidance and particle control demand that an analogous regime

be identified for core barriers. This once again illustrates the importance of understanding barrier dynamics.

4) The parameters (local and global) determining barrier width and depth. This is quite fertile territory.

It is clear from this discussion that the capacity for flexibility is essential to a science program. The ability to vary and compare different shapes, current profiles, profiles, etc. is needed to understand the cause-and-effect relationships.

The ability to compare different transport barrier regimes under various conditions is needed to assess their relative performance, e.g.,

1.) ELM-y H-mode edge barriers versus reversed-shear core barriers. Specific points of comparison include thresholds, hysteresis factors, stability and disruptivity, and particle control.
2.) VH-mode internal barriers (controlled via edge fueling) versus reversed-shear mode.
3.) The optimal transport barrier mode for high density, compact regimes.

D.9 Attachment 2 to Major Tokamak Facilities Appendix
Power and Particle Exhaust

In a commercial fusion reactor based on the burning of D-T fuel in a magnetically confined plasma, the plasma facing structures will need to handle substantial heat and particle loads, and exhaust thermalized helium ash. In a tokamak reactor, the configuration most likely to satisfy all of these requirements will be some form of poloidal divertor.

In a tokamak, the scrape off layer, consisting of the plasma on open flux surfaces outside the separatrix, diverts the power and particle exhaust away from the reacting central plasma to remote material surfaces, the divertor plates. This plasma also serves as a buffer zone between the core and first wall, to isolate the cold surfaces from the hot plasma and to screen impurities from re-entering the core plasma. The key engineering challenges which must be met for successful operation of the divertor, and therefore the reactor, are:

1) Power dissipation. Channel and dissipate the core power exhaust over surrounding surfaces in such a way that those surfaces are long-lasting, which requires minimization of surface heat loading and erosion.
2) Impurity control. Impurity levels in the reacting plasma must be kept very low; deleterious effects of impurities include volumetric power loss due to radiation (line and continuum) and dilution of the reacting fuel. Estimates show that if high-Z contamination (tungsten, for example) were to exceed 10^{-5} of the electron density, line radiation would prevent ignition. In ITER, with an effective charge (Z_{eff}) of ~1.5 due to low-Z impurities and helium, calculations show that 30% of the input power to the plasma (including alpha power) would be radiated by free-free bremsstrahlung. Aside from the helium ash produced by the fusion reactions, the main natural sources of impurities are the plasma facing surfaces, primarily in the divertor itself. Moreover, it may also be necessary to introduce impurities artificially into the scrape-off plasma, in order to satisfy the power dissipation requirement. In all cases, the ability of the divertor plasma to prevent these impurities from reaching the core reacting plasma will be crucial.
3) Helium exhaust. Excessive helium buildup in the core plasma would destroy its reactivity because of fuel dilution. Once the alpha particles have thermalized, and assuming they are transported to the edge of the plasma across the separatrix, it is the goal of the scrape-off and divertor plasmas to keep the helium from returning to the core (goal 2) and to maximize the helium concentration in the divertor, thus maximizing the capability of pumps connected to the divertor to exhaust helium from the system.

The areas of physics research associated with the above goals are:

a) perpendicular and parallel transport (hydrogenic, He and impurity species, energy and momentum);

b) atomic and molecular physics (charge-exchange, ionization, radiation, momentum balance, shielding);
c) magnetic boundary control (x-point, separatrix-wall intersection point and gaps); and
d) plasma-material interactions (sputtering, recycling, particle ballistics).

The end result of successful divertor physics and engineering studies will be the ability to make credible predictions of ITER/reactor performance by:

a) Extrapolation/interpolation to reactor conditions through a combination of experiments, numerical modeling (benchmarked to experiments), and scaling arguments. This requires both achievement of parameters as close as possible to those of ITER, as well as data over a wide range of parameters (magnetic field, physical dimensions, density, wall material, divertor geometry) to test theory and modeling.
b) Demonstration of the mutual compatibility of the goals (1-3), simultaneously with good core plasma performance. The conditions required for good divertor operation are somewhat in conflict with those required for enhanced core confinement. The current strategy to dissipate power in the divertor requires a cool dense divertor plasma, perhaps with sufficient impurity content to increase radiation. Efficient He pumping also requires a high divertor density. On the other hand, the transition to H-mode seems to require a hot plasma edge and relatively low density plasmas. A reactor would require very low impurity levels to minimize fuel dilution and radiation. Wall conditioning is another area where further development is needed. Up until now, most, if not all enhanced confinement modes are in some way dependent on, or improved by changes in edge conditions. Examples include H-mode (diverted), H-factors and VH-mode (boronization), and super-shots/high T_i (lithium wall conditioning). Enhanced core confinement and self-consistent alignment of pressure and current profiles for large bootstrap fraction will require control of density profiles, and thus recycling and particle pumping can be expected to play important roles.

What is the status of these studies to date? There are many encouraging results for goals 1-3. However there has been no demonstration of full compatibility even at relatively low power flow levels, much less at reactor-like conditions.

A number of foreign tokamaks (JET, JT-60U, ASDEX-U) are actively studying divertors. In addition, both Tore-Supra and TEXTOR are studying pumped limiters and ergodic magnetic limiters as alternate plasma boundary concepts. Most divertor designs use carbon plasma facing components, though JET has used beryllium and ASDEX-U is installing tungsten-coated carbon. JET, JT-60U, and ASDEX-U are modifying their divertors to designs that approximate the ITER design, to address ITER R&D issues.

In the U.S. program, high power dissipation has been demonstrated on DIII-D and Alcator C-Mod. In C-Mod, this has been done in the ITER divertor geometry at the expected divertor densities and within a factor of two of the expected parallel power flow levels. C-Mod is the only divertor experiment in the world using all-metal plasma facing components, which will most likely be required in a reactor to bring erosion within acceptable limits. DIII-D has demonstrated successful pumping of dissipative divertor operation and is the only tokamak in the world that will operate a pumped double null divertor with variable slot length.

D.10 Attachment 3 to Major Tokamak Facilities Appendix
Profile Control and Auxiliary Heating Physics and Technology

The need for auxiliary heating of magnetically confined plasmas in order to achieve thermonuclear temperatures has been recognized and studied experimentally for a long time. In addition to providing the means to reach ignition, auxiliary heating has also been found to enable operation in enhanced confinement regimes, such as H-modes or Supershots, and the formation of internal transport barriers. Methods devised for delivering power to plasmas include neutral beam injection (NBI) as well as various schemes based on interaction of externally launched electromagnetic waves with the plasma. Neutral beam injection differs significantly from wave heating in that it is also a fueling source for the plasma. Its utility as a central heating source in tokamak reactors is limited because beam penetration to the plasma core decreases as the density and temperature of the plasma increase. Research and development of neutral beams based on negative-ion accelerators, which feature higher injection energy and therefore greater penetrating power, is underway in Japan and Europe and may lead to a beam injection system suitable for use in reactors. Because of the difficulties with existing NBI technology, most reactor designs have featured some combination of electromagnetic wave heating. Outstanding issues regarding wave heating that remain to be addressed include the performance of these methods in deuterium-tritium plasmas and compatibility of hardware design with reactor environments.

Noninductive current drive methods have also been studied for a long time because a combination of self-generated bootstrap current coupled with external sources of noninductive current is required for steady state tokamak operation. In the high beta, high fusion reactivity plasmas envisioned in advanced tokamak configurations, the total plasma current will be dominated by the bootstrap current while the pressure profile will be dominated by the alpha heating profile. However, since the bootstrap current profile is determined by both the pressure gradients and the poloidal field profiles, and since the overall MHD stability of the plasma is determined by the net current and pressure profiles, while the fusion reactivity is mainly determined by the pressure profiles, operation in this regime is inherently nonlinear. One must create and maintain a plasma with self-consistent pressure and bootstrap current profiles which simultaneously provide maximum fusion reactivity, MHD stability, and steady-state operation. The auxiliary input power used for control must be minimized in the interest of overall plant economics. Because the bulk of the plasma current is provided by the bootstrap current in these regimes, external current drive will be used in initiating operation and to fine-tune the current profile locally.

Methods for core pressure profile control are critical, since the pressure profile influences the fusion reactivity, the plasma stability and the bootstrap current. Methods that have been demonstrated include boundary control via plasma shaping, divertors, or wall conditioning; particle fueling; current profile control; and flow shear generation using ion Bernstein waves.

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Proposed methods for core pressure control are limited at this time to schemes for controlling the core transport, such as internal transport barrier formation with ion Bernstein wave heating or controlling the alpha heating profile via alpha channeling. Considerable further research is necessary to establish the utility of these proposed schemes for future reactors. In the case of alpha channeling, operation in a deuterium-tritium plasma is required to either confirm or disprove the utility of the scheme.

Appendix E

Plasma Confinement Research (Alternative Concepts)

The study of plasma science, and the many scientific issues which underlie fusion research, is greatly aided by experiments in which a range of confinement configurations is employed. A key feature of a laboratory experiment is that the experimenter has some control over the plasma conditions, permitting a controlled study of plasma phenomena. However, the range of variation of key parameters in any given plasma configuration is limited. Study of fusion science issues in different confinement configurations greatly expands the range of physical conditions which are accessible; such an approach leads to enhanced understanding and innovation which would not be attainable through the investigation of one confinement concept only. For example, from one concept to another variations occur in magnetic curvature, magnetic shear, fluctuation spectra and amplitudes, electric fields, plasma current, plasma flow, plasma pressure, and many other properties. Moreover, the quest to optimize the properties of a magnetic configuration, with regard to fusion reactor attractiveness, leads scientists to investigate a range of configurations. For example, alternative concepts research aims for smaller size, higher plasma pressure, less plasma current, less plasma disruptions, and lower magnetic field.

Since fusion research worldwide is focused on the tokamak, confinement concepts sufficiently different from the tokamak (or advanced tokamak) have come to be called "alternative concepts." In addition, within the alternative concepts, different magnetic configurations, often differing by only small changes in magnetic field structure, are known by different names. This categorization understates the strong scientific connections among the different concepts, and their cross-fertilization. Inertial confinement fusion is a major, separately funded program (within Defense Programs in DOE), with comparably strong international activity. It is discussed in the next appendix. The decision to concentrate resources on tokamaks, beginning around 1970, was made because confinement times were superior to those achieved in other magnetic configurations at that time. Of course the gap widened in time. In 1990, in response to budget pressure, the alternative concepts program was essentially terminated in favor of schedule-driven development of the tokamak reactor concept. A significant element of the restructured program will be a reinitiation of an alternative concepts research program.

The status of most alternative concepts is such that pressing scientific issues can be addressed in experiments of modest scale. Thus, this is one area in which the United States can participate at the forefront worldwide. The very strong alternative concepts programs in Japan and Europe are focused on the stellarator. In other concept areas the international effort, although much stronger than that in the United States at present, is distributed over medium scale and small experiments. There is a spectrum of alternative concepts which are arguably ready for modest-scale experimentation and theoretical study, exploiting advances in theory/modeling and experimental techniques which has occurred in recent years. FEAC has not attempted to evaluate the scientific value or reactor significance of different alternates. However, for illustration we describe below the status and opportunities of five magnetic confinement concepts.

Conclusions: *Alternative concepts offers opportunities to advance fusion science in ways not possible with one concept only, to pursue configurations with possibly attractive reactor features, to excel in selected areas with modest expenditure (even in the context of a constrained overall fusion budget), and to encourage new ideas -- all goals aligned with the restructured fusion program.*

Recommendation: *The concept improvement program should be expanded to include a spectrum of alternative concepts, including experimental and theoretical research. Several concepts may be ready for experiments to elucidate key physics issues. The precise funding level cannot be prescribed here. It must be driven by peer-reviewed proposals (from national labs, universities, and industry), as for any scientific program. As for the program's major facilities, any experiments which are operated should be supported with healthy funding to operate cost-effectively.*

E.1 Field Reversed Configurations

Description and Strengths. A Field Reversed Configurations (FRC) is an elongated compact toroid (torus with unity aspect ratio, i.e., no central hole) with negligible toroidal field. It is thus extremely high β (minimum possible value of 50%) and is thought to rely upon a combination of kinetic effects and velocity shear for its observed stability and robustness. FRCs may have extremely desirable attributes, being nearly linear in external geometry while providing internal toroidal confinement. Only a simple, low field, solenoidal external magnet (essentially the vertical field coil of a tokamak) is required. A natural divertor with linear exhaust out of the ends allows for simple heat removal and possible direct conversion. The extremely high β provides for a compact design and the possibility of burning advanced, aneutronic fuels.

Status. The confinement and stability physics of FRCs are necessarily quite different from that of tokamaks since there are large, nearly field-free regions and no magnetic shear. The observed stability of small scale experiments had been ascribed to kinetic effects represented by the parameter s (the effective number of ion gyro-orbits between the field null and the separatrix), but recent results in the Large s Experiment (LSX), demonstrating robust stability at s values as high 4, have led to a rethinking about the stabilization mechanism. Minimum energy states (akin to those in Spheromaks and RFPs, but involving the total canonical angular momentum rather than magnetic helicity alone) have been calculated. These involve velocity shear as a stabilizing mechanism, which is also being studied in tokamak devices. Since compact toroids have no ohmic transfer, even quasi-steady-state operation must be provided by RF or neutral beam current drive. Current drive involving bulk electron acceleration by Rotating Magnetic Fields (RMF) has been demonstrated in small scale, cold FRC experiments (rotomaks). The physics involved in such a method is extremely interesting, involving basic questions of electron orbits and collisionality parameters (affecting slippage off the rotating flux), and would illuminate fundamental processes in plasma turbulence. RMF current drive is very efficient, and could be beneficial to tokamaks as well as FRCs. It can be seen that study of the above features not only provides breakthrough possibilities in reactor engineering, but their study will also lead to new understandings of fundamental fusion plasma physics and technology.

LSX was only operated for one year due to the decision to terminate all alternative concept research. However, in that time impressive plasma parameters of over 1 keV ion and 0.5 keV electron temperatures were reached, and stable operation at s values over 4 were demonstrated. Confinement parameters of $n\tau$ (product of plasma density and energy confinement time) over 10^{12} s-m^{-3} also have been achieved. Relatively low voltage formation techniques were developed, to the point where flux build-up to large sizes through neutral beam or RMF application can be conceived as a reactor relevant technique.

Scientific Challenges/Opportunities. A principal outstanding question in FRC development is that of maintaining stability as the size increases to that required for reactor level confinement, s ~ 20-30. Recent theoretical calculations indicate that velocity shear (which will not diminish with increasing size) may be a more important contributor to stability than kinetic orbits, but this has not yet been verified experimentally. Energy transport remains an additional key area for future study. The old LSX facility was transferred to the University of Washington and modified to conduct translation experiments related to tokamak fueling. This modified facility is ideal for continuing FRC investigation at modest cost, particularly to studying the effects of external profile controls and steady state current drive in a simple chamber separated from the high voltage formation section. If the RMF current drive technique can be successfully applied to pre-existing hot FRCs, then high s values can also be achieved in the same device. This would provide an extremely cheap path toward reactor development, which is probably a necessity in today's limited budget environment.

E.2 Reversed Field Pinch

Description and Strengths. The reversed field pinch (RFP) is similar to the tokamak, except that the toroidal magnetic field is about ten times smaller than that of a tokamak (for similar plasma current). The reduced field yields a reactor concept possibly characterized by high power density, high plasma beta, low forces on magnet coils, non-superconducting coils, absence of disruptions, and free choice of aspect ratio (chosen by engineering, not physics, constraints). The benefit of these features has been demonstrated in several reactor studies.

The RFP is particularly useful for the study of fundamental fusion physics issues such as the dynamo effect (related to the astrophysical dynamo problem), magnetic fluctuation induced transport, nonlinear mode coupling, resistive wall effects on MHD modes, helicity injection, current profile effects on fluctuations, electrostatic fluctuations in bad curvature and high shear systems, and mode locking to field errors. The RFP, FRC, spheromak, ST (and the advanced tokamak) are all aimed at more compact reactors with weaker magnetic field. The RFP and spheromak attain the low field by operating at safety factor $q < 1$. If the confinement can be made favorable in this regime, as discussed below, then a new domain of parameter space is made available for toroidal reactors.

Status. Significant RFP experiments are in operation in four laboratories worldwide: MST at the University of Wisconsin, RFX in Italy, two devices in Japan, and T2 in Sweden. MST and RFX are the largest devices, and of similar size (minor radius ≈ 0.5 m). RFX has higher current capability (2 MA versus about 0.5 MA for MST). The four RFP laboratories are well-coordinated, with complementary work underway in each lab.

RFP experiments operate at high values of beta (~ 10%); however, significant advances are necessary in energy confinement. Recently, substantial progress has been made in confinement understanding and improvement. In MST, transport specifically driven by magnetic fluctuations has been measured. Toward the plasma interior magnetic fluctuations drive transport, as anticipated theoretically. Detailed comparison of magnetic fluctuation properties with nonlinear MHD computation indicates good agreement. Hence, reasonable understanding of transport in the RFP may be emerging.

This understanding suggests that fluctuations and transport can be reduced through control of the current density profile. This has opened up a new route to confinement improvement. Initial attempts with inductive current profile control in MST have succeeded in tripling the energy confinement time, from about 1.3 ms to about 4 ms (and modestly reducing fluctuations). This result encourages the more sophisticated methods described below. The fluctuations are part of the well-known dynamo effect, or self-generation of magnetic field. It has been confirmed in experiment that the MHD dynamo (similar to that of astrophysics) accounts for the edge current of collisionless RFP plasmas. Interestingly, in the edge of collisional RFP plasmas, a new dynamo mechanism driven by pressure fluctuations has been discovered.

Particle confinement in the extreme edge has been measured to arise from electrostatic fluctuations, as in a tokamak. However, the cause of energy confinement in the extreme edge remains unknown.

Theory has played an important role in RFP understanding. Nonlinear MHD theory and computation yields a comprehensive understanding of many features of the magnetic fluctuations, the dynamo, and the mean fields. In addition, MHD has been used to examine the effect of a resistive wall on stability, helicity injection current drive, the effect of current drive on fluctuations, the effect of finite pressure on fluctuations, and the scaling of fluctuations with Lundquist number. Description of the RFP and plasma relaxation by the minimum energy state approach has been very useful for RFPs and other plasma venues.

Transport by magnetic fluctuations has been studied by analytic turbulence theory (MHD codes cannot treat energy transport in the collisionless regime), which has provided insight into the effects of ambipolar constraints on the transport of current and energy. Studies of RF current drive (lower hybrid and fast wave) for the bulk plasma and for current profile control, has been initiated with early results indicating, for example, that lower hybrid waves are suitable for profile control for transport suppression.

Scientific Challenges/Opportunities. Four key areas are confinement improvement, bulk current drive, resistive wall stability, and power exhaust. Confinement improvement can proceed along two paths. First, empirical scaling trends might imply that confinement will improve with current. The goal of RFX is to test this conjecture to 2 MA. The MST experiment will study confinement improvement by current profile control. Three techniques are planned: inductive current profile control, electrostatic current injection (helicity injection), and lower hybrid current drive. These experiments require a strong theoretical complement in MHD computation, fundamental turbulent transport analysis, and RF current drive calculations.

For MHD stability the RFP must be surrounded by a conducting shell. The shell requirements have not been determined experimentally, nor have means for alleviation such as plasma rotation. The T2 experiment in Sweden plans to address this issue.

Steady state current drive has not yet been studied extensively for the RFP. The RF problem is akin to that of the ST, in that the toroidal magnetic field is low. However, the RFP does not benefit from bootstrap current. This is an important area for further theoretical research and new ideas. Two techniques which have been suggested from theory, fast wave current drive and oscillating field current drive (in which oscillating loop voltages produces a net current), require experimental study.

Confinement, resistive wall stability, and current drive can be advanced aggressively in experiments of modest scale. A program in RFP theory should include work in these areas.

E.3 Spherical Tokamak

Description and Strengths. Spherical tokamaks have low aspect ratio, in the range A ~ 1.1 - 1.5. By definition they are compact magnetic confinement devices. The steep 1/R variation of the toroidal field allows high plasma current operation at low-medium safety factor with small toroidal field. Other characteristics include natural elongation, good vertical stability to position displacements, good MHD stability at high beta, and natural plasma edge exhaust. All the usual advanced tokamak operation modes are also available in STs.

The above-mentioned features of STs makes them an attractive candidate for either a fusion reactor or a volume neutron source.

From a scientific point of view, important physics issues can be resolved in STs because in them the cylindrical q value is very different from the flux surface q (for instance, one can distinguish current scaling from q scaling for confinement or Troyon coefficients). Neoclassical theory and much of MHD may need reassessment because the large A approximation is no longer valid.

Status. Ideal MHD properties of STs have been extensively explored by theory. Neoclassical effects in the extreme low A limit have shown that a bootstrap current may exist even in the collisional limit. Preliminary studies of RF current drive need some refinement for the low A regime. Gyrofluid simulations of transport arising from ion-temperature-gradient-driven turbulence are underway. Many numerical tools developed for standard tokamaks are applicable to STs (equilibrium, time dependent equilibrium and transport, MHD, kinetic solvers, etc.).

There are presently three very small experiments in the United States (CDX-U at Princeton, HIT at the University of Washington, and Medusa at the University of Wisconsin) investigating spherical tokamaks, and one somewhat large ST in England (START, which has delivered particularly interesting results). Experimental results from these low current STs indicate further benefits of low aspect ratio: absence of disruptions, expanded scrape-off layer widths, large shaping, good confinement -- all of this observed in a hot plasma with a reasonable pulse length. Internal reconnection events (described as sawteeth or minor disruptions by some) reduce the discharge performance, but do not destroy the plasma. They are not understood at present. Helicity injection can produce low current plasmas. Reactor studies based on extrapolations of the present performance of START indicate that an ST reactor may have the following properties: low plasma thermal and particle diffusivities, order unity beta, order unity fraction of well-aligned self-driven current, absence of plasma disruptions, a highly dispersed plasma exhaust channel, small currents in toroidal and poloidal coils.

Scientific Challenges/Opportunities. How do the characteristics of STs scale with aspect ratio? In particular, how does confinement (core and edge) scale? Is low disruptivity a characteristic of low current plasmas or of STs? Can we produce a non-inductively driven tokamak plasma? Will current drive requirements force excessively high betas and disruptions?

A new device with higher plasma current and auxiliary heating would allow exploration of the potential advantages of STs in more fusion relevant regimes. A reasonable next step would be a device with about 1 MA of plasma current. Smaller devices can contribute to specific issues, as well as innovative ST research.

Non-inductive current drive is essential to STs, since there is little space for a solenoid. Innovative current drive techniques may need to be developed (helicity injection, fast wave, for instance). The small surface-to-volume ratio of the ST, characteristic of small, compact reactors, may create very high heat loads on plasma facing components. These difficulties are shared with advanced tokamaks.

Specific to STs are the design of the TF and PF systems. To maintain low aspect ratio, the TF central leg has to be small, hence resistive (it would have to be replaced often in the presence of neutron damage). Detailed shape control may be difficult, since poloidal field coils are not allowed inboard of the plasma, and internal current redistribution in the plasma can greatly affect the plasma boundary.

E.4 Spheromak

Description and Strengths. Spheromaks are compact toroidal configurations including both toroidal and poloidal magnetic fields, with the fields maintained by plasma currents rather than external coils linking the torus. The spheromak potentially has many attractive reactor features. It is compact, with high energy density. It is the simplest device which has a toroidal field, while the lack of a hard-core conductor potentially makes a reactor maintainable and capable of a low cost-of-electricity. Electrostatic helicity injection can allow steady-state operation, and other current drive options may be possible. Low edge toroidal field can facilitate divertor design as well as reduce coil costs. Such compact toroids allow the plasma to be transported, which has both reactor and plasma fueling applications. The spheromak can be viewed as an RFP in the limit of unity aspect ratio. There may be similarities in the transport physics since both concepts have safety factor less than one. It differs from the FRC only in that it contains a substantial toroidal magnetic field.

Status. Spheromak experiments achieved core electron temperatures of 400 eV, comparable to the results of the T-3 tokamak in 1969 which led to the rapid increase in the world tokamak program. The ion temperature in spheromaks was higher, the density 10 times higher, but the global confinement time much less. These results came as the result of understanding the importance of several physics issues, including the role of a conducting wall, of field errors and edge physics effects, and of MHD stability.

Spheromak research has made many other physics contributions to fusion research. It was recognized for many years that the magnetic geometry is predicted by the Taylor-Wells theory of energy minimization at constant helicity. Now recognized are the many similarities in "relaxation" physics between spheromaks and reversed field pinches and the importance of helicity generation, conservation, and dynamo effects. And accelerated spheromaks or compact toroids have been used successfully to inject particle density in tokamak experiments.

Scientific Challenges/Opportunities. The spheromak confronts many of the issues faced by the RFP, particularly confinement and current drive, as well as beta limits (optimization of magnetic shear to maximize the Mercier limit). There are a number of key physics topics which can be studied in a spheromak experimental program at relatively low cost. One path would start with a short-pulsed, hot spheromak to study energy confinement in a sustained plasma. This experiment would also be used to study shaping of the plasma to maximize beta and to provide initial data on the transition from a plasma equilibrium supported by a conducting shell to one supported by external coils. Success in this experiment would lead to a long-pulse experiment to demonstrate multi-keV equilibria supported by external coils, control of low-n instabilities, long pulse helicity injection, and other steady-state issues. Another avenue of potential gain is to develop current drive techniques for profile control to suppress fluctuations and transport, similar to the research plan for the RFP.

E.5 Stellarators

Description and Strengths. The stellarator confining magnetic field is produced entirely by currents flowing in external conductors. It does not require a plasma current. Consequently, stellarators can be intrinsically steady-state reactors and avoid the problems associated with current-driven disruptions, current drive, high bootstrap fraction and positional stability. The lack of current leads to low circulating power. Other reactor advantages include a natural helical divertor and a lower power density on the divertor plates than for a tokamak. To date, no density limits have been observed. Finally, control of the magnetic geometry and rotational transform by means of external coils allows for the possibility of enhanced confinement that is more robust than in a tokamak. Magnetic properties, such as shear, well depth, and localization of particles to the good curvature region can be "hard-wired" into the magnetics.

Status. Stellarator design has advanced such that experiments are now designed from the inside out: the boundary shape is calculated based on a set of prescribed physics properties and a set of helical or modular coils is then determined which produces the desired boundary. This pioneering computational methodology, known as the HELIAS (Helical Advanced Stellarator) approach, was developed in Garching, Germany.

The worldwide stellarator program contains many stellarator configurations. Two large steady-state stellarators with superconducting coils are under construction at a cost in the range of $0.5 - 1.0 B each: LHD, a torsatron with helical coils in Japan; and Wendelstein 7X, an advanced stellarator with modular coils in Germany. An advanced stellarator, W7-AS, operating in Germany, was designed to have a lower Pfirsch-Schluter current than a tokamak or conventional stellarator. A heliac is a class of stellarators with a large magnetic well, with circular coils arranged toroidally about a central conductor so that the center of each coil lies along a helix. A three-field period heliac is operating in Australia (H-1) and a four field period heliac (TJ-II) will begin operation in Spain at the end of 1996. Two low aspect ratio torsatrons are in operation: CHS in Japan and CAT at Auburn University in the United States. HSX, under construction at the University of Wisconsin, is a quasi-helically symmetric stellarator with magnetic properties similar to that of a tokamak (but achieved without plasma current). Conventional stellarators with helical windings are in operation in Japan, Russia, and the Ukraine.

A major liability of the stellarator had been the poor confinement of trapped particles at low collisionality. Experimentally, it has been observed that large neoclassical transport in this regime can exceed anomalous losses. The HELIAS approach to optimizing magnetic configurations was developed in part to overcome these limitations. Neoclassical transport in W7X, for example, is expected to be greatly reduced. The mission of the HSX device is to verify the improved neoclassical transport properties. A difference in the magnetic properties between W7X and HSX is the relative size of the bootstrap current. W7X is designed to minimize the bootstrap current so that finite beta effects will have minimal impact on the magnetics. Confinement scaling in stellarators is similar to that in tokamaks, yet to date significant progress has yet to be made in finding improved confinement regimes. H-modes have been observed, however the energy confinement improvement is small.

Scientific challenges/Opportunities. The optimal trade-off between neoclassical transport, simplicity of coil design and bootstrap current remains to be determined. In addition, study of anomalous transport (including trapped particle effects) is needed to discover the optimal combination of shear and curvature. The role of electric fields, and concomitant flow, is particularly important to confinement in the stellarator.

Another issue is whether there exist magnetic configurations which offer a possible route to higher beta than presently envisioned for a stellarator reactor (about 5%). Theoretical work is needed to understand the ideal MHD stability of stellarators and the breakup of magnetic surfaces from finite pressure effects. Experimental beta values of 2% have been achieved. Exploration of higher beta regimes await TJ-II and LHD.

The U.S. role in the world stellarator program should encompass three research elements (as follows, but not listed in priority order). First, a stellarator theory program, formerly an area of excellence in the United States, could play an important role in the world program. Second, innovative experiments of modest scale can influence the world program. The flexibility of the magnetic configuration of the stellarator offers opportunities to explore variations in coil structure, magnetic field spectrum, aspect ratio, beta stability limits, and improved confinement properties. For example, the geometry of HSX is unique, such that the planned experiments cannot be performed in the large stellarators in Europe and Japan. Third, the United States will benefit significantly from participation in the substantial physics experiments abroad.

Appendix F

Inertial Fusion Energy

In Inertial Confinement Fusion (ICF), the fusion energy is released by imploding a small pellet of deuterium and tritium using energetic lasers or particle beams as drivers. The physics of pellet implosion up to the point of ignition and burn is complex due to the interplay of hydrodynamic motion, electron and radiation transport, equations of state under extreme conditions, and the appearance of instabilities. Basically, the energy from the driver explodes the outer layer of the target, causing an implosion which pushes the fuel towards the center of the pellet and compresses the fuel to densities of several hundreds of g/cm^3. Fusion is initiated in the central region of the pellet (called the hot spot) which then drives a burn front through the remaining target fuel. The hot spot is crucial for obtaining high gain (high ratio of fusion energy released to driver energy), because only a small fraction of the total fuel has to be driven to fusion conditions in the hot spot to initiate fusion burn of the remaining fuel in the pellet.

During the target implosion, high-density material is accelerated by a layer of lower density material. Their interface is subject to the Rayleigh-Taylor instability. Ignition can only be achieved if the distortion of this interface layer is small. Therefore, high-gain targets require a high degree of spherical symmetry in the implosion (< 1%-2%). The direct-drive approach in which the pellet is heated directly by the driver requires this degree of symmetry to be achieved in the target illumination. Significant progress has been made in this area and is expected in the next few years from the Omega facility. Another solution is the indirect-drive approach which converts the incident beam energy into soft x-rays in a hohlraum which then symmetrically drive the implosion of the pellet. The drawback of this scheme is the inefficiency of the conversion process. The pellets also cost more, which may be a factor if the pellet fabrication cost becomes a major cost-driver item for IFE.

Inertial confinement fusion is now the largest fusion science program in the United States and is primarily supported by Defense Programs in DOE. The principal near-term purpose of the ICF program is stockpile stewardship, to provide the scientific base for nuclear security applications. The DOE, through Defense Programs, has approved the National Ignition Facility (NIF), a billion-dollar ICF facility that is designed to demonstrate ignition in ICF pellets by about 2005. Study of the high spot and burn in NIF will also settle the main scientific issues of high-gain targets and establish the driver requirements.

Four different drivers have been identified and studied for ICF purposes: glass-based lasers, KrF lasers, light-ion accelerators, and heavy-ion accelerators. The development of the first three drivers was funded by DOE Defense Programs, and the glass-based lasers were chosen for the NIF. Heavy-ion accelerators have been investigated for Inertial Fusion Energy (IFE) power applications, because for these energy applications the drivers must be reliable and efficient with a high pulse repetition rate (several Hertz) and long life. In this regard, heavy-ion accelerators have been deemed as the most promising driver option for IFE. It should be noted that significant progress has been made during the past few years both in KrF lasers (impressive smoothing and high bandwidth) and in glass lasers with diode pumping.

It should be noted that the primary approach to heavy-ion ICF and glass-laser-based NIF is the indirect-drive approach. At the same radiation temperature, x-ray hohlraum wall losses, radiation-driven hohlraum wall motion, and radiation transport for laser-driven hohlraums are directly applicable to heavy-ion ICF. These are the primary issues that affect coupling efficiency and hohlraum symmetry for the heavy-ion ICF hohlraums. Because of these similarities, Defense Programs ICF work provides a solid basis for calculating the most critical elements of the heavy-ion target. Therefore, the national IFE program is highly leveraged on the large national ICF effort. Internationally, energy applications of ICF are pursued in Europe and Japan and international collaboration in this area has increased substantially after the recent declassification of most of the ICF program in the United States.

In its early years, heavy-ion-based IFE received funding from high-energy physics in the Office of Energy Research and from ICF in the Defense Programs. Beginning in FY84, heavy-ion IFE work was concentrated in the Heavy Ion Fusion Accelerator Research (HIFAR) Program in Basic Energy Sciences. In 1990, the Fusion Policy Advisory Committee (FPAC) to the Secretary of Energy stated "The promise of an inertial fusion energy program (IFE) seems to us to be sufficient to begin investment now in a small collateral program covering those areas not required for the Defense Programs, e.g., repetition-rated, efficient drivers and reactor studies." As a result of FPAC recommendations, the HIFAR Program was moved to the Office of Fusion Energy and became the primary element of the new IFE program. Noting the target physics will be mainly supplied by the Defense Programs ICF, the IFE program at OFE aims at addressing the following scientific issues: 1) Development of a suitable heavy-ion driver; and 2) Driver-independent issues for IFE such as chamber wall protection and design, affordable and mass-produced targets, power plant and integration studies to identify key critical issues, etc. The FPAC also recommended a large increase in the funding for the national fusion program that did not materialize. As a result, the funding for IFE research (at OFE) has remained relatively flat since 1990 and the work is mainly focused on the heavy-ion accelerator development.

Many reviews of the IFE program during the past few years have indicated that the accelerator development program is ready to proceed to the next step -- the ILSE project, which would provide an integrated demonstration of induction linac technology and the beam physics required to provide the data base for scaling to a heavy-ion driver for an inertial fusion power plant. In

1993, the Fusion Energy Advisory Committee Panel 7 on Inertial Fusion Energy reviewed the status of the IFE program in the Office of Fusion Energy and recommended a "balanced program that includes an experimental and analytical program for supporting IFE technologies as well as accelerator development and a beam physics program." This panel recommended a "reference" annual budget of $17M for IFE with $14M identified for ILSE and accelerator research, and $3M for supporting technology and system studies. The panel also found that for $10M per year ($8M for accelerator research and $2M for supporting technology) it is not possible to complete the integrated ILSE project, although a significant set of large accelerator experiments could be completed to increase understanding of key technical issues.

As a whole, the recommendation and conclusion of the FEAC Panel remains valid today. The IFE program annual budget of $8M is focused on accelerator development and related beam theory. At this level of funding, the heavy-ion IFE program cannot proceed to its logical net step (i.e., the ILSE project). This budget level also reflects the fact that there is limited scientific synergy between accelerator development and magnetic fusion. However, IFE and MFE share a large number of scientific issues: MFE plasma science and IFE driver-independent plasma science issues (funded in Defense Programs), and fusion support technologies of both IFE and MFE.

The SciCom did not assess the IFE effort in detail, but acknowledge its potential as a fusion energy source and the major role of DOE Defense Programs in addressing key scientific and plasma physics issues. A programmatic review should be conducted involving all cognizant DOE program offices, and appropriate scientific and technical experts to recommend the priority and management of IFE, in the context of the mission, policy, and scientific goals of the restructured fusion energy sciences program.

Appendix G

International Thermonuclear Experimental Reactor (ITER)

G.1 Need for a Burning Plasma Physics Experiment

It is not possible to demonstrate the full physics basis that will be required for fusion power reactors in the present generation of large tokamaks. The critical issues that cannot be resolved in these facilities relate to the formation and control of burning deuterium-tritium (DT) plasmas. In this context a key parameter is the energy multiplication factor,

$$Q \equiv \text{(total fusion power)} / \text{(net auxiliary heating power)}.$$

Four fifths of the fusion power from DT reactions escapes from the plasma as 14 MeV neutrons, while 1/5 of the power is returned to the plasma in the form of 3.5 MeV alpha particles. Hence, Q must be greater than 5 for the self-heating of a burning DT plasma to dominate the auxiliary heating. An economic fusion power reactor must operate in the strongly self-heated regime with $Q \gg 5$. Issues relating to the control of the plasma operating point and plasma profiles may be qualitatively different in this strongly self-heated regime. It might be possible to reach Q of about 2 in the largest U.S. facility (TFTR); in an international context similar efforts in JET (the only other tokamak in the world planning substantial DT experiments) might lead to qualitatively similar results. Since neither of the present generation of DT experiments is likely to reach the self-heated regime, a necessary follow-on is a burning plasma experiment -- that is, the demonstration of controlled ignition and extended burn of a DT plasma.

Goals for a burning plasma physics experiment include both the demonstration of long-pulse ignition -- that is, the maintenance of a burning DT plasma without the application of significant auxiliary heating power -- and the demonstration of driven operation at high plasma pressure (as measured by $\beta \equiv 2\mu_0 p/B^2$), allowing steady-state (as opposed to pulsed) tokamak operation in which the plasma current is supported through a combination of non-inductive current drive (which requires the application of auxiliary power) and self-generated currents (that is, the "bootstrap-current," which is a well-known consequence of neoclassical transport theory). The ignition goal reflects the view that tokamak optimization is best achieved through simplifying the reactor concept by removing all unnecessary elements (like the auxiliary heating and current drive systems). In contrast, the goal of achieving a high-β, steady-state driven burn builds on the concept innovation program being pursued at our large tokamak facilities, and reflects the view that the intelligent application of modest amounts of auxiliary power will allow greater control over the plasma, leading to greatly increased fusion performance relative to what might be achieved in self-ignited operation. The new physics issues that must be addressed to achieve these two goals mainly relate to fusion alpha physics, dynamic control of the operating point, control of plasma profiles (through non-inductive current drive and the formation and control of transport barriers), particle and heat removal, and disruption avoidance and/or mitigation.

G.2 The Role of ITER in the U.S. Program

Given the high projected cost of a burning plasma physics experiment, and the fact that the US presently funds only about 1/6 of the world effort in magnetic fusion, we concur with the existing program strategy:

Burning plasma physics will be pursued through international collaboration.
At present, the principal international collaboration aimed at the construction of a burning plasma experiment is the ITER EDA -- a collaboration involving the United States, the European Community, Japan, and the Russian Federation.

The ITER mission is "to demonstrate the scientific and technological feasibility of fusion power. The ITER will accomplish this by demonstrating controlled ignition and extended burn of a deuterium and tritium plasma with steady state as an ultimate objective, by demonstrating technologies essential to a reactor in an integrated system, and by performing integrated testing of the high-heat-flux and nuclear components required to utilize fusion power for practical purposes."[1] This mission addresses both the physics and technology issues essential to an engineering test reactor. An engineering test reactor, of which ITER is an embodiment, has been in all the Parties' fusion development plans. It is the penultimate step in the development of a particular fusion energy concept. If successful, ITER would be followed by a demonstration reactor. This final step would demonstrate the reliable production of electrical power and provide a basis for utilities to evaluate the financial viability of fusion power plants based on the tokamak concept. Accomplishing this commonly required step internationally reduces the costs to the U.S. and the other Parties, while drawing upon the competence of all the Parties enables the highest quality team possible to implement the project. If ITER is successful, it will reduce the time required and total development cost of fusion power at the price of increased risk and greater near-term cost.

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[1] *Establishment of ITER: Relevant Documents,* (IAEA, Vienna, 1988).

ITER is both a great physics challenge -- if ITER is built, it will test burning plasmas at the reactor scale and provide a test bed for most of tokamak physics, and much generic physics -- and a great technological challenge, requiring the development of high-field superconducting magnets, high-heat-flux components, plasma heating and fueling systems, and other reactor-relevant fusion technologies. U.S. industry has been given a major role in designing and building prototype components of the ITER during the EDA, while having access to all design and development activities of the other parties. This role helps to assure that American industry will be able to compete for construction elements, if ITER is built and the United States participates. In addition to the potential benefits of sharing in the ITER construction cost, this important international collaboration focuses the world fusion program on a concrete objective.

G.3 U.S. Participation in the ITER EDA

The U.S. program extracts substantial benefits from the ITER EDA. At present, ITER is the primary vehicle for plasma technology development in the U.S. program (as discussed in the Materials and Technology Appendix). The Physics R&D plan developed by the ITER project (in consultation with the home teams of the ITER parties) provides one of the foci for our large tokamak experiments. Important physics issues must still be resolved in the areas of divertors, disruptions, density limits, transport scaling, L-to-H and H-to-L mode power thresholds, current and pressure profile control, and β-limits. These issues are generic to tokamak fusion reactors and, so, must be addressed ultimately with or without the ITER project if we are to develop fusion reactors based on the tokamak concept. However, The ITER EDA has added urgency to the international effort to address these issues, motivating a substantial increase in the international effort in some areas (e.g., divertor physics, and the characterization of disruptions), while providing a mechanism for very substantial increases in the international dissemination of relevant data from all ITER parties in other areas (e.g., transport scaling and H mode power thresholds).

G.4 Review of ITER EDA

The results of the ITER physics R&D, together with the ITER physics design requirements will form the physics basis for ITER. This *ITER Physics Basis* will be assessed by all parties before there is a decision to construct ITER. In particular, the U.S. program will review the ITER EDA output (including both physics basis and engineering design) prior to a decision to seek participation in ITER construction. Given the pivotal nature of a decision on ITER construction to the U.S. program, we recommend that a mechanism be established immediately to expand involvement of the U.S. fusion community in the assessment of the evolving ITER physics and technology basis to insure that the ITER design reflects our current best understanding of tokamak physics, and to insure that the U.S. community appreciates the issues that have driven ITER design decisions. Given the time pressure that FEAC is acting under in preparing this report, we suggest four avenues for achieving expanded community involvement in the assessment of the ITER physics basis and design, while recognizing that further refinement of these suggestions may be necessary:

1) The U.S. Home Team should make an effort to increase contact with members of the U.S. fusion community through presentations and informal conversations at meetings, making themselves available to present talks on ITER at universities and national laboratories, and making an effort to attend presentations by others that relate to ITER. Similarly, members of the U.S. fusion community should seek to open and maintain communication with members of the U.S. Home Team and colleagues on the ITER JCT.

2) The ITER Joint Central Team (JCT) has developed a framework for interacting with the worldwide tokamak science community via the overview ITER Physics Committee and seven ITER Physics Expert Groups in the areas of:

1) Confinement and Transport Physics,
2) Confinement Database and Modeling,
3) Disruptions, MHD, and Plasma Control,
4) Energetic Particles, Heating and Current Drive,
5) Diagnostics,
6) Divertor Physics, and
7) Divertor Database and Modeling.

These expert groups provide a formal channel through which the U.S. program can raise concerns which have developed regarding the ITER physics basis. We can make better use of them by insuring that there is adequate funding provided to U.S. members of these expert groups to allow them to spend substantial amounts of time preparing for meetings of the group and disseminating the information obtained at these meetings. In particular, U.S. members of these expert groups should take responsibility for seeking out areas of legitimate concern with the ITER physics basis within the U.S. fusion community, communicating this concern to the ITER project, and facilitating a dialogue between concerned members of the U.S. community, experts from other ITER parties, and members of the ITER JCT.

3) The U.S. ITER Home Team already has an oversight committee, the ITER Steering Committee U.S. (or ISCUS). The ISCUS snould be encouraged to take an active role in defining key issues that must be resolved prior to a U.S. decision on participation in ITER construction, evaluating plans to address these issues in a timely manner, and working with their home institutions to see that appropriate efforts are made to resolve technical issues in a timely manner.

4) The ITER detail design will be available by the end of calendar year 1996. The U.S. program should consider launching an assessment of the ITER detail design modeled after the European Domestic Assessment of the ITER Interim design -- that is, it should be performed by a working group composed of about 50 experts drawn from the U.S. fusion program. While it should include members of the U.S. ITER Home Team (who, being familiar with the ITER design and physics basis, can assist other members of the working group in understanding the ITER design choices), the majority of the members should not be associated with the U.S. ITER Home Team, and should represent all elements of the U.S. program (universities, national laboratories, and industry). Subgroups should be formed to address major issues, and the assessment should extend over a period of several months (involving multiple meetings and/or teleconferences between members of the subgroups) to allow time for a thorough evaluation of the technical issues before the full working group prepares a final report.

G.5 Strategy Regarding ITER Through the Construction Decision

The recent budget changes in the U.S. fusion program indicate that the United States is very unlikely to participate as a full partner in ITER. Nevertheless,

- **we recommend that the U.S. program continue participation (as allowed by the budget) in the EDA phase of ITER to which the United States is committed through FY98,**

thereby fulfilling our existing commitments to our international partners, and leaving open the possibility of some U.S. participation in ITER construction or other major international collaborations which provide a cost-effective means of advancing fusion science.

In the remainder of the EDA, it is important for the U.S. program to increase its emphasis on advanced tokamak scenarios to insure that the ITER facility provides a suitable vehicle to pursue the study of tokamak concept innovation -- the current focus of our tokamak experimental program -- in a burning D-T plasma. This effort would include increased attention to operational flexibility, the definition of the ITER heating and current drive systems, diagnostics, and control systems. These are areas in which the U.S. program has expertise, and ones in which the United States might have maximum impact at minimum cost during ITER construction and operation.

Regardless of the outcome of the post-EDA phase, the United States will have benefited from our involvement in the ITER EDA, because ITER has acted as a driver for technology development, engineering design innovation, and for involving the world in an enhanced collaborative attack on the primary physics and operational issues of tokamaks. It has also forced the fusion community to face squarely the engineering challenges of designing a steady state, high power, DT burning plasma device.

In 1998 the EDA agreement will be concluded and the parties will be faced with a decision on ITER construction. Since ITER construction is expected to cost in excess of $6 B, it is clear that the U.S. program cannot participate as an equal partner at present budget levels. However, the European, Japanese, and Russian programs have indicated that they may welcome U.S. participation as a limited financial partner. If ITER is built, then ITER will set the world standard for reactor-relevant fusion plasmas. If the United States is not able to participate in ITER operation, then our experimental fusion program will lag behind this world standard. Some may object that ITER will not demonstrate the "scientific and technological foundations for an economically and environmentally attractive fusion energy source." However, it is our present best understanding that:

1) The advanced steady-state operating modes that we presently envision as the route to tokamak concept improvement can be demonstrated in ITER.

2) Our ITER partners are prepared to modify the ITER design to accommodate such advanced operating modes as the relevant physics database becomes available.

3) If we are allowed to participate in ITER as a limited financial partner for about $50M per year, then the ITER device will be the most cost-effective means for the U.S. program to obtain the physics database on advanced operation of burning DT plasmas that would be required before the construction of a demonstration reactor based on such an operating mode. (If construction

takes 10 years, our construction cost is only about $500 M -- far less than any credible alternative that the United States might build on its own.)

Hence, if our ITER partners agree to pick up the bulk of the cost of ITER construction, and the U.S. program agrees that the ITER design complements our goals, then we should

- **seek to participate in ITER construction as a limited financial partner**

to provide a continuing focus for our tokamak physics and technology program, so that U.S. scientists will have access to the ITER facility after it is completed, and so that the United States can benefit (to an extent commensurate with our contributions) from lessons learned in ITER construction and operation. Possible U.S. contributions to ITER construction include:

Diagnostics and control systems. At the end of the ITER EDA much work will remain to be done in the design of diagnostics and plasma controls, which are crucial to extracting the maximum amount of information regarding burning plasma physics and advanced operating modes during ITER operations. The U.S. Home Team presently has lead responsibility for design of the ITER diagnostics systems. We might seek to build on the existing capability of the U.S. fusion community in the area of plasma diagnostics by retaining leadership for diagnostic design and seeking a leading role in fabricating the ITER diagnostics and control systems. This will insure a strong role for the U.S. experimental physics community in ITER operations; it will build on the existing leadership of U.S. industry in the area of computers and control systems; and it will position the United States to champion the concept of remote experimental sites for ITER operation. The presence of one (or more) ITER control rooms within the United States would allow substantial involvement of the U.S. experimental community in ITER operations while minimizing the costs and personal hardships associated with relocation of U.S. physics personnel to the ITER site.

Final ITER Design work. Continued involvement of the U.S. physics community in the final ITER design work (e.g., developing disruption control systems, modeling divertor operation, etc.) and in the preparations for ITER operations (e.g., developing shot plans for ohmic, ignited, and advanced tokamak operational scenarios consistent with the design limits of ITER systems and expected plasma performance) would also help to insure a strong role for the United States in ITER operations.

Magnet instrumentation. An area in which the U.S. program has unique capabilities is the instrumentation, protection control, and data acquisition for the magnet systems. The United States has a niche leadership in this area, and this activity would give global access to fabrication details and components, like strand and jacket material.

Divertor Cassettes. The cost for building the prototype, the 60 production divertor cassettes, and the spare parts for nine additional cassettes is about $200M in current dollars. This would be an affordable key system for the United States to manufacture and supply to the project. It is an area where the United States has the lead in the EDA and has the most experience in actively-cooled in-vessel components.

Fueling Systems. The cost for manufacturing the gas and pellet injection fueling system for ITER is approximately $40M. This is another area where the United States has the lead in the EDA R&D program and, in fact, the United States is the only party developing the high velocity tritium pellet fueling systems required for ITER. This would be a natural system for the United States to supply.

A possible outcome of negotiations over US participation in ITER construction is that the U.S. fusion community will have little control over how the U.S. contribution to ITER construction is spent. Even in the absence of a direct scientific and technical role for the U.S. fusion community in ITER construction, we should still consider participation in ITER construction in order to gain access to the ITER device after it is constructed: so that U.S. experimentalists can participate in the planning, execution, and analysis of experiments in a long-pulse, ignited plasma; so that U.S. industry can benefit (to some extent) from the experience in fusion-relevant technologies obtained in the construction and operation of ITER; and to further the development of the "scientific and technological foundations for an economically and environmentally attractive fusion energy source."

G.6 What if ITER Isn't Built?

We foresee three basic avenues for continued international collaboration on fusion if the ITER partners decide not to construct ITER but remain interested in further collaboration with the U.S. program. These are:

1) An international collaboration to design and construct a less ambitious (and costly) device aimed at achieving ignition. A design study of a long-pulse (~100 seconds) ignition tokamak

based on liquid nitrogen cooled copper magnets was recently completed in response to suggestions made in the report of the PCAST committee. This device had a major radius of 5 meters, an aspect ratio R/a = 3.3, a plasma current of 15 MA, and a projected cost of between 44% and 60% of the ITER device. The BPX/CIT project developed the design of a short-pulse ignition device (about 7 seconds), and found a cost of about 1/4 to 1/5 that of the ITER device. The IGNITOR project, taking a more aggressive approach to the engineering limits, concludes that short-pulse ignition can be achieved in a high-field liquid-nitrogen cooled tokamak at a cost less than 1/10th that of ITER. While a short-pulse ignition experiment based on any of the designs mentioned above may prove technically feasible, it is important to note that our ITER partners have either shown no interest in such suggestions, or have been actively hostile to suggestions that the present ITER design be abandoned in favor of such a device. Hence, persuading one, or more, of our ITER partners to join us in this project would be a major undertaking.

2) An international collaboration to design a very-long pulse (1000 seconds to "steady-state") driven tokamak. This might be something like the TPX tokamak (total project cost was estimated at $700 M); or it might follow on the more ambitious Japanese JT60-super upgrade design which envisioned operation in both deuterium and DT. Project goals might include demonstration of steady-state plasma operation and control in both conventional and advanced tokamak operating modes (similar to the goals of the recently canceled TPX project).

3) A less ambitious approach would be to defer advances in plasma confinement and control, and focus international collaboration on the construction of an International Fusion Neutron Source. Such a facility is estimated to have a cost of about $1B in as-spent dollars.

The present sentiment within the fusion community strongly favors pursuing the most ambitious of these alternatives -- a burning plasma experiment. Hence, a low-level U.S. domestic effort to search for less expensive means of studying burning plasmas would be useful insurance against the possibility that ITER is not constructed. Note that any such effort could not be part of the U.S. contribution to the ITER EDA. Should that search prove successful and in the event the international partners decide to modify their objectives for a next step device, then the United States should explore with its former ITER partners (and other nations if this becomes appropriate) the possibility of international collaboration on a less expensive means of fulfilling the goal of DT ignition and burn. However, we must recognize that we cannot expect to take international leadership in a project for which we are not willing to accept an equal financial commitment with prospective partners. We must pay careful attention to the views of possible partners, and be prepared to accept their leadership in the event that they choose to pursue a different avenue to advance fusion science and technology.

Appendix H

Fusion Materials and Technology

Fusion science encompasses fundamental materials and technology development including low activation materials and fusion technologies essential to achieve the safety and environmental potential of fusion, and critical, enabling plasma technologies required to support advances in plasma physics. Historically, advances in fusion materials and technology have been a driver for progress in plasma physics. Fusion materials and technology have also made major scientific contributions through multi-discipline exploration of new phenomena in the unique fusion environment. The three major research areas are: 1) low activation materials; 2) fusion technologies; and 3) plasma technologies.

H.1 Low Activation Materials

It is widely recognized that achievement of the safety and environmental potential of fusion energy requires the successful development of low activation materials for components immediately surrounding the plasma. To attain high performance, these materials must satisfy complex operating requirements including temperature, stress, chemical environment and radiation exposure. Materials applications include structural materials, tritium breeding materials, electrical insulator materials, and plasma facing materials. Because of the radiation damage issues and the complex operating requirements, materials development is recognized as being a long-term endeavor. In addition, there exist only a limited number of materials which exhibit low activation characteristics along with a potential for high performance and long lifetime. Although materials development is usually presented as a separate issue because of the unique expertise required, it is important that materials development for fusion be closely coordinated with the respective component, e.g., blanket, divertor, etc. A key to the success is development of a compatible combination of materials for each component.

The emphasis in this section is on the structural materials application. The issues related to tritium breeding materials and plasma facing materials are included in the following sections on fusion technologies and plasma technologies. A 14 MeV neutron source for materials testing will eventually be required to fully qualify materials for fusion applications. The issues associated with this materials test facility are also summarized below.

Structural Materials

Development of high performance, low activation structural materials is recognized as one of the key issues in the development of fusion energy. In addition to the safety and environmental considerations, development of low activation structural materials is a priority since the critical issues are well defined, the issues are common to all plasma confinement concepts, and materials development is a long term endeavor. The performance requirements for the structural materials are unprecedented. The operating requirements and materials issues involved are shown in Table 1 on page H-3..

The materials science associated with these requirements must be understood to determine performance and lifetime limitations of candidate materials with low activation characteristics. The structural materials effort is currently limited to only three candidate materials: ferritic steels, SiC/SiC composites, and vanadium alloys.

Ferritic Steels. The ferritic steels under consideration are a modified composition of a conventional iron-9%chromium steel. Molybdenum in the conventional steel has been replaced by tungsten and certain trace elements have been reduced to improve the low activation characteristics. Favorable characteristics relate to proven manufacturing and joining technology for the conventional steels, resistance to oxidation, and resistance to void swelling. Major issues include potential irradiation embrittlement at the lower temperatures of interest, limited high temperature creep strength, and effects of ferromagnetic properties. Development of a suitable electrically insulating coating will be required for application in a liquid metal blanket option.

SiC/SiC Composites. SiC/SiC composites represent a highly advanced structural ceramic material with unique low activation characteristics and high temperature properties particularly applicable to helium-cooled ceramic breeder blanket options. Favorable characteristics of SiC/SiC composites relate to favorable short- to medium-term low activation and high temperature mechanical properties. Major issues relate to the limited data base. Further development is required to determine its: suitability as a structural material including effects of neutron irradiation and high helium transmutation rates on swelling, thermal conductivity and mechanical properties; fabrication and joining technology; gas permeability due to porosity and micro cracking; compatibility with breeder materials; and hydrogen retention characteristics.

Vanadium Alloys. Vanadium alloys with 4-5% chromium and titanium represent an advanced alloy option with a limited but encouraging data base, particularly applicable to liquid-metal-cooled blanket options. Favorable characteristics associated with vanadium alloys include very low long-term radioactivity, excellent thermal stress capacity and creep strength, resistance to void swelling, and resistance to irradiation embrittlement. Major issues relate to sensitivity to oxidation by gaseous impurities and atmospheric exposure, development of stable insulating coatings for liquid metal coolants, lifetime under irradiation, fabrication and welding development, and compatibility with helium coolant and ceramic breeder materials.

Table 1. Operating Requirements and Materials Issues

Operating Requirements	Materials/Issues
Radiation damage including displacement damage and transmutations	Swelling, irradiation creep, and degradation of physical and mechanical properties
Chemical compatibility with coolants, tritium breeding materials and plasma facing materials	Corrosion, mass transfer, degradation of mechanical properties & hydrogen interactions
Elevated temperatures	Thermal creep and reduced mechanical strength
Mechanical stresses including primary, thermal, cyclic, and high strain rate (disruption) stresses	Mechanical properties including tensile, fatigue, and fracture toughness
Fabrication of complex structures	Fabricability and welding/joining
Plasma interactions	Thermomechanical response including plasma compatibility, vaporization/melting during disruptions, sputtering, tritium retention and ferromagnetism

Development of a fundamental understanding of the performance limits of these materials for the fusion conditions will require a major effort. A key issue in all cases relates to the effects of simultaneous helium transmutations and displacement damage produced by high energy (14 MeV) neutron exposure. Irradiation data are currently obtained from fission reactor irradiations which produce lower energy neutrons, and hence, much lower helium transmutation rates. Since a high flux 14 MeV neutron source for materials testing is not available, the effects of helium must be determined by artificial simulations. These techniques provide valuable information on the influence of helium transmutations on the materials responses such that important advances in the development of materials can be made; however, as discussed later, a 14 MeV neutron source will be required to fully qualify materials for a fusion power plant.

The current materials development program makes extensive use of international collaboration, particularly with respect to the irradiation facilities and sharing of developmental materials. The materials efforts in Europe, Japan and Russia are complementary with extensive sharing of results. This type of internationally coordinated program should continue in order to make maximum use of available resources.

Non-Structural Ceramics

Requirements for non-structural ceramics, particularly electrical insulators and diagnostic materials, are less well defined; however, it is important to develop a fundamental understanding of selected phenomena that will be important to various fusion applications. The relatively limited data available regarding radiation effects on non-structural ceramics gives cause for serious concern for their use in the fusion environment. At relatively low fluences some candidate insulators appear to degrade markedly in resistivity, and fused quartz normally used in RF windows and fiber optics becomes opaque.

The current program also makes extensive use of international collaboration to conduct difficult irradiation experiments which require in-situ electrical measurements. This type of collaboration should continue and close coordination with the ITER project should be maintained to guide in the development of priorities and requirements.

Conclusion: *Development of high performance, low activation materials is essential to achieve the safety and environmental potential of fusion energy. Because of the demanding operating requirements and radiation damage issues, materials development is recognized as a long-term endeavor. The low activation materials program should be continued with extensive international collaboration.*

Neutron Source for Materials Testing

Fission reactors provide a valuable means for simulating the atomic displacement of a fusion radiation environment; however, the 14-MeV neutrons produced by the fusion reaction cause significant transmutation reactions that do not occur in a fission reactor spectrum. Although fission reactor irradiations do not fully simulate the effects of a fusion spectrum, the results are valuable in developing an understanding of the effects of irradiation on the properties of materials. In addition, for some materials, additional simulations have been devised to mimic the effects of certain transmutations, particularly helium transmutations, which occur at a much higher rate in a fusion spectrum and are known in some cases to have a major effect on the properties of materials.

Previous reviews of the materials programs (Fusion Policy Advisory Committee Report of 1991 and FEAC Report of 1993) concluded that a 14-MeV neutron source for materials testing was a critical element of the program and that the need would shift from desirable to essential in about the same time frame as ITER construction. The 14-MeV neutron source was considered necessary for three reasons: 1) to confirm predictions of materials performance obtained from fission-reactor irradiations; 2) to complete development of advanced materials; and 3) to extend the engineering design databases on materials performance to the goals for fusion damage levels. These conclusions were based on the previous fusion program strategy for development of a fusion DEMO by about 2025. Implementation of this strategy included initiation of a conceptual design study (CDA) of a 14-MeV neutron source to be carried out by international collaboration under the auspices of the International Energy Agency. It was agreed by the international community that an accelerator-based D-Li source was the most appropriate concept for this purpose. An interim conceptual design of this neutron source has just been completed as a joint effort of the European Community, Japan and the United States. A cost estimate of this device is currently being developed as part of the CDA and should be available in a few months. Since preliminary information suggests that the cost will be in the range of $0.5-1 B, it is difficult to see how the United States could contribute a significant share of the construction cost under the present budget. It is recommended that the U.S. materials program address this problem over the next year. This should be done with involvement of the international community. Possible resolutions include downsizing of the facility, participation as a limited financial partner, or seeking funding outside of the fusion program.

Conclusion: *A 14-MeV neutron source for materials testing, developed as part of the international fusion program, has been a major element of the U.S. program. Under the revised strategy with a constrained budget, it is recommended that the United States, jointly with its international partners, re-evaluate the priorities of this facility.*

H.2 Fusion Technologies

The safety, environmental attractiveness, and economic competitiveness of fusion energy will depend to a large extent on the blanket system since this is the largest component exposed to the high neutron fluence. The overall goal of the fusion technologies area is to develop a blanket system that meets the performance requirements of tritium self-sufficiency and efficient energy recovery, and at the same time achieves the desired level of safety and environmental attractiveness. Meeting this goal involves several possible design concepts, a number of candidate materials, and a variety of scientific disciplines. The development of blankets is at a relatively early stage so there is a considerable amount of research still required before key issues can be resolved and a suitable blanket system can be defined. The design concepts can be conveniently divided into blanket systems that use liquid metals as breeding materials and/or coolants and blanket systems that use lithium-bearing ceramics (solid breeders) as tritium breeding materials. The issues and required research for each of these categories, including tritium systems and safety-related research, are briefly described below.

Liquid Metal Blankets

There are two liquid metals, lithium and a PbLi alloy, that have been identified as having the greatest long term potential. Both liquid metals provide good tritium breeding capability, and both can also be used as coolants in self-cooled concepts. It is also possible to use a separate coolant, such as helium or water (with PbLi only), and the liquid metals then only need to be circulated at a low rate sufficient to remove the tritium bred during operation. The leading candidate designs for pure Li as the breeder/coolant use a vanadium alloy (V-4Cr-4Ti) as the structural material. Designs for PbLi have used ferritic steel as the structural material. Within the United States, research on liquid metal blanket concepts has focused on the self-cooled concept with pure Li as the coolant/breeder and V-4Cr-4Ti as the structure. This system combines the features of a low activation system with high temperature operation (650-700 C) which translates into an efficient energy recovery system. The self-cooled concept operates at low coolant pressure (~1 Mpa) and has a relatively simple design layout that offers the potential for high reliability and long component lifetime.

There are several issues that need to be resolved in order to demonstrate the promise of the Li/V option:

Materials -- Self-cooled blankets require the use of an electrically insulating coating material on the inside of cooling channels to reduce the MHD pressure drop of flowing liquid metals in the high magnetic fields characteristic of fusion power systems. The coatings can be quite thin (1-10 mm) but must be have sufficient electrical resistivity to effectively limit the flow of currents in the structure. They must be thermodynamically stable in the liquid metal environment and should be self-repairing in case cracks or defects are formed during operation. The research items that are to be addressed are determination of the conditions and kinetics of the formation of the candidate coatings, chemical compatibility and corrosion in liquid lithium, and irradiation effects on the electrical resistivity and microstructure.

Magnetohydrodynamics (MHD) -- The flow behavior of liquid metals in high magnetic fields is dominated by MHD forces for which there is limited understanding for systems with insulated walls in fusion-relevant magnetic fields. Research needs include demonstration of the effectiveness of insulator coatings and fundamental studies of the flow patterns that develop in coolant channel geometries typical of the fusion blanket. The flow pattern studies are important to the understanding of heat transfer behavior in self-cooled blankets.

Safety -- Liquid Li is chemically reactive with air and water, and therefore special safety considerations are necessary to insure safe operation. Research is needed to determine the behavior of liquid Li during possible accidents. Information to be obtained includes chemical reaction rates in different possible environments.

Tritium -- Tritium is highly soluble in liquid Li, and the most challenging aspect of the tritium system is identifying a suitable means for separating the tritium from the Li. For PbLi, the primary challenge is to insure tritium containment. Development of a tritium barrier is a key issue for PbLi blankets with water as a coolant. Research is needed to determine the absolute tritium solubility as a function of temperature and to identify approaches that can be used to effectively remove the tritium so the inventory in the system can be maintained at acceptable levels.

The work that has been performed in the United States is complementary to liquid metal work in other countries. The European Community has emphasized the development of the PbLi option, both self-cooled and separately cooled. The United States has focused on the self-cooled Li concept and U.S./E.C. collaborative programs have been underway since 1987 in the areas of MHD research and insulator coating development. The Russian Federation has been investigating both the Li and PbLi options. U.S./R.F. collaborative programs on blanket technology have been underway since 1989.

Solid Breeder Blankets

There are several solid breeder materials that have been considered, including Li_2O, $LiAlO_2$, Li_4SiO_4, Li_2ZrO_3, and Li_2TiO_3. The desirable features are good thermal conductivity and neutron energy multiplication for efficient energy recovery, a net tritium breeding ratio greater than one, and rapid release of tritium at typical blanket operating temperatures. There are a number of chemical properties that affect tritium release including tritium bulk diffusivity, surface recombination and release, and chemical interaction with the purge gas. Design studies have indicated that an effective neutron multiplier is required to provide tritium self-sufficiency in the solid breeder blanket concepts. Beryllium is the primary candidate as a neutron multiplier to enhance the tritium breeding capability. All these materials should remain stable over an extended time in the fusion environment. The coolants being considered in solid breeder designs are helium and water, with helium being preferred for long-term applications. The structural material most often used in solid breeder designs is ferritic stainless steel; however, SiC/SiC composites provide an attractive structure option for He-cooled solid breeders concepts if development proves successful. In the United States, the leading solid breeder concept uses Li_2TiO_3 (similar to Li_2ZrO_3) as the breeding material, beryllium as the neutron multiplier, reduced activation ferritic stainless steel as the structure, and helium as the coolant. Li_2TiO_3 is believed to have excellent tritium release characteristics, appears to be quite stable during neutron irradiation, and it is a low-activation material; however, the data base is very limited. This blanket concept is similar to others being considered in the European Community and Japan. The ceramic breeder development program has involved extensive international collaboration, particularly with the European Community and Japan.

There are several issues that need to be resolved in order to demonstrate the promise of the solid breeder options:

Materials -- The behavior of the physical and mechanical properties of candidate solid breeders and beryllium, both before and after irradiation, needs to be better understood. In addition, research is needed on the chemical compatibility between the blanket materials and the structure at fusion relevant conditions.

Heat Transfer Characteristics -- Heat transfer across interfaces between solid materials is crucial to developing a blanket that provides efficient energy recovery and that maintains the material temperatures in the optimum operating range. The unique materials and complex geometries of the fusion blanket result in a limited understanding of interfacial heat transfer. Research is needed to determine heat transfer across interfaces as a function of material morphology, surface characteristics, and interfacial stress. The effects of irradiation on these properties must also be determined.

Safety -- Beryllium at elevated temperatures is chemically reactive with air and water, and therefore special safety considerations are necessary to insure safe operation. Research is needed to determine the behavior of beryllium during possible accidents. Information to be obtained includes chemical reaction rates in different possible environments.

Tritium -- In contrast to liquid metals, with solid breeders no coolant is circulated during operation so the tritium bred during operation must be released in the blanket itself. Beryllium will also produce tritium albeit at much lower rates than in the solid breeders. Tritium release in beryllium is an important issue; over a period of time this tritium can add substantially to the blanket tritium inventory. Over the years, there have been several reactor experiments that have explored tritium release in solid breeder materials at normal operating temperatures. Additional research is needed to explore the tritium release characteristics at the limits of temperature operation to establish the operating windows for the leading candidates after high fluence irradiations. Tritium release characteristics of beryllium are less well understood than in solid breeders, and thus research on beryllium is also required.

The work that has been performed in the United States is complementary to solid breeder blanket work in other countries. The European Community has emphasized the development of the $LiAlO_2$ and Li_4SiO_4 options, but most recently has been investigating Li_2ZrO_3. U.S./E.C. collaborative programs have been underway since 1986 in the areas of solid breeder characterization and irradiation response. Japan has been investigating the Li_2O option, but recently has begun to investigate the ternary ceramics. U.S./Japan collaborative programs have been underway since 1986.

Conclusion: *The safety and environmental attractiveness of fusion will depend to a large extent on the blanket system since this is the largest component exposed to a high neutron fluence. Development of a blanket system that meets performance requirements of tritium self-sufficiency and efficient energy recovery, while meeting the safety and environmental goals remains a critical issue in the development of fusion energy.*

Tritium Systems

Tritium systems development is critical to the development of fusion. Tritium systems must be developed for processing tritium in the fuel cycle as well as for processing tritium from the blanket system. The tritium systems for the fueling system dominate the tritium systems. The United States has provided a lead role for several years in the development of tritium processing systems. The Tritium Systems Test Assembly (TSTA) in the United States is a world class facility for tritium processing. This technology has been transferred to the TFTR facility for the current DT test program. Extensive international collaborations with the Japanese have been conducted with the TSTA. Development of tritium systems for fusion applications should be continued. Future work should include further development of the fundamental aspects of blanket processing systems since this area has received only limited attention.

Safety-Related Research

Safety-related research is also an important part of the fusion technology program. Since a key aspect of the development of low-activation materials and technology for the U.S. fusion program relates to the safety goals of fusion energy, it is important to maintain this element of the program to guide and evaluate the safety benefits. The United States has provided leadership in the international community in this area, including contributions to the ITER project. It is important that the safety programs be closely coordinated with the materials and technology programs to provide maximum benefit. Continued involvement with the international safety effort is important to development of guidelines and rules for application to fusion systems.

H.3 Plasma Technologies

Plasma technologies are defined as those enabling technologies that have impact on plasma containment (magnetics), plasma control (heating and fueling), and plasma output (power and particle handling). The application of new plasma technologies has been essential to the advancements in fusion. In the 1960s hot plasmas were of short duration with many impurities, and the state of the vacuum vessel walls was not characterized. During the 1970s auxiliary heating systems provided controlled multi-kilovolt plasmas. More recently, high field copper and superconducting magnets, improvements to fueling, proper choice of plasma facing materials, and wall conditioning techniques have led to significantly higher temperatures and longer pulse lengths in tokamaks and other configurations.

ITER is presently the primary vehicle for plasma technology development in the U.S. program. The United States has the lead in two of the seven major ITER deliverables: the central solenoid model coil and the divertor cassette prototype. It also plays an important role in other ITER plasma technologies. However, any current or next generation device benefits from the U.S. plasma technology R&D. The basic U.S. concept of the "cabled" niobium-tin superconductor is embodied in the design of superconducting fusion devices. Heating and fueling systems are required for any magnetic confinement concept. In Electron Cyclotron Heating (ECH), the United States holds the world record for pulsed gyrotron energy output and CW operation. Our Ion Cyclotron Heating

(ICH) antennas are found on numerous fusion devices around the world. The United States is also preeminent in pellet fueling technology. Finally, plasma-material interactions are intrinsic to all magnetic fusion concepts. The United States is a leader in actively cooled plasma facing component development.

Superconducting Magnet Technology

The primary goal of the magnet program is the development of niobium-tin superconductors and radiation-resistant insulation systems for reliable operation in magnetic confinement device environments. The purpose of the program is to provide one of the enabling technologies for future devices. Superconducting magnets are a requirement for long pulse and steady state future devices.

The major development issues have been to establish high confidence in the long term reliability of components, and to assure stable conductor operation under pulsed field and disruption conditions. The United States has a special long-term expertise to contribute in these areas. For example, the conductor and fabrication concepts adopted by ITER were originated under pre-ITER U.S. base program activities.

The current application of the U.S. generated concepts is far broader than ITER. For example, the basic concept of "cabled" niobium-tin conductors is embodied in the design of all current superconducting tokamaks -- TPX, JT-60 Super-Upgrade, and the Korean STARX. With retention of a strong magnet technology infrastructure, and good-faith participation in the EDA, the United States would be welcome in any such future international device.

Maintaining a base program is required for long term continuity. Projects generally contribute to maturity of a technology, but projects are rarely organized to incubate or sustain development. Developments generally arise and are optimized in "base programs" -- projects then carry only the last stage development and/or scale-up, and drop out as soon as they have what they need.

The laboratories and universities provide continuity of knowledge, innovation, and characterization of superconducting strand, structural materials, helium technology, and conductor behavior. Many of the more than 100 university graduates trained over the years in these areas supported by the OFE magnet programs have found jobs in the industrial superconducting infrastructure.

Superconducting strand fabrication requires a continuity of industrial production. The Japanese have been very successful in their industrial production because they maintained a continuity through their base program. For the United States to remain competitive, we believe we must retain a continuity of industrial wire fabrication, and we must understand and optimize that production by basic work in the university/laboratory infrastructure.

Plasma Facing Component Technology

Plasma facing component technology is a fundamental enabling element of any fusion science program. Plasma materials interaction issues are present in all magnetic fusion devices. The interaction between the plasma and the material which faces it is the single most significant interaction that limits both the advancement of fusion science and the development of the fusion energy option. Any probable improvements in fusion concepts result in smaller, steady state devices which will stress plasma facing materials technology beyond its present capacity to withstand heat loads on the order of 5 MW/m^2. Utilization of any future improvements in plasma performance is also inextricably linked to improvements in plasma facing materials.

The goal of plasma facing component (PFC) technology is the development of reliable, steady-state components that can handle the heat and particle emission from the plasma without contaminating the plasma. The technology issues include: enhanced heat removal technology; development of plasma facing materials (PFMs) and joining technologies for attaching PFMs to heat sink materials; assessment of thermal and mechanical fatigue; development of in-situ repair techniques; development of non-destructive examination techniques for assuring reliability of components; establishment of a database; and models for plasma-material interactions.

Plasma facing component development requires the use of specialized facilities capable of addressing the issues. The minimum, most cost-effective set of test facilities required for development of reliable, long-lived PFCs consists of high power density tokamaks, small linear plasma simulation devices, ion/electron beam high heat flux test stands, pulsed, high power density plasma guns for simulation of disruption thermal loads, and fission reactor for neutron irradiation damage studies. The United States has world-class facilities in each of these areas: DIII-D (DiMES), PISCES and TPE, EB-1200, PLADIS, and HFIR. All of the U.S. tokamaks and future device designs (including the TPX design) and several international machines have used these facilities to develop their plasma facing components. Plasma facing component R&D has led to improvements in low Z materials, understanding of erosion, wall conditioning techniques, hydrogen effects in materials, and enhanced heat removal (all crucial to plasma performance).

The United States is the world leader in plasma facing component development. It is leading the international effort to develop ITER divertor components. The U.S. plasma facing component technology program has collaborated on the design and fabrication of components for TFTR [Carbon-Fiber-Composite (CFC) materials], DIII-D (CFC materials), Tore Supra (steady-state heat-removal components), TEXTOR (pumped limiters), JET (beryllium plasma facing material), JT-60U (CFC materials), and LHD (stellarator PFCs). The components (inner-wall limiter, pump limiter, and ergodic divertor dump plates) on Tore Supra are the only steady-state components in any operating tokamak. They were designed for 2 MW of heat removal and a peak heat flux of 15 MW/m^2 based on experience gained from short pulse devices. The U.S. PFC community is involved in tokamak edge plasma diagnostics, analysis of samples from tokamaks for surface contamination, tritium/deuterium retention, failure modes studies, and erosion measurements.

Possibilities for continuing the technology development beyond the work in progress include development of reduced activation materials and novel concepts for heat removal. Concept improvement and advanced concepts will likely lead to higher heat fluxes that are more challenging technically. This implies a need for advanced plasma facing materials (liquid surfaces, infiltrated surfaces) to improve component lifetime. Safety concerns about water cooling imply the need for developing alternate coolants (liquid metals, helium gas). Erosion and tritium studies are needed for advanced materials to address safety issues. Methods for rapid in-situ repair will increase the useful lifetime of plasma facing components.

Ion Cyclotron Heating (ICH) Technology

The goal of the ICH technology program is the development of reliable, high-power (≥ 50 MW), steady-state plasma heating and current drive systems. These systems can be used for:

- efficient and cost-effective core heating of plasma ions (or electrons);
- current drive to enable long-pulse or steady-state tokamak operation;
- current profile control to access and sustain advanced operating modes (e.g., reversed-shear).

They are currently being used for these applications on a number of U.S. and international machines.

ICH is a significant part of the R&D programs on major confinement experiments both here (DIII-D, TFTR, and C-MOD) and abroad. Support from the ICH base technology program enables U. S. RF scientists and engineers to have a unique role in the world program, in which knowledge gained from one experiment can easily be applied to another. Examples of such synergism are:

- Fast tuning and matching R&D -- we are collaborating on the design, analysis, and testing of the JET and DIII-D RF control systems, as well as working on the design of the ITER tuning and matching system.

- Long-pulse antennas — we have furnished a new water-cooled long-pulse RF antenna (which has demonstrated excellent characteristics in initial testing there) to Tore Supra, and are collaborating with the Tore Supra staff on RF experiments. Experience with such antennas influences the design and fabrication of antennas for other machines (e.g., DIII-D), and is also being used in our design studies for the ITER antenna.

- Advanced technology — we are supplying advanced components to international experiments such as ASDEX-U, CHS, W7AS, and TEXTOR, and collaborating with their researchers to evaluate the components' performance.

Knowledge gained from fusion ICH R&D is also being applied to non-fusion applications of RF-generated or heated plasmas, such as plasma processing of semiconductors and other materials

There are several key development issues for ICH. Fast tuning and matching of multiple-strap, high-power antennas in the presence of changes in the plasma properties (e.g., from ELMs or plasma motion) is a high priority for both present-day and future applications, especially ITER. R&D in this area is well underway on DIII-D, JET, and other machines. The development and testing of long-pulse or steady-state advanced antennas (e.g., the folded waveguide) with much higher RF power capabilities (>20 MW/m^2; reliable operation at 10 MW/m^2) will make ICH much more attractive due to the reduced number of ports required to deliver a given amount of power.

The United States has particular expertise in antenna design, and in calculation of the interaction of the large RF fields from the antenna with the core and edge plasma regions. In addition, we are leaders in the technology of antenna and RF component fabrication, and have developed advanced methods of antenna construction. The United States can take a major role in the design and construction of the ITER ICH system. Expertise exists in laboratories and industry to supply a cost-effective, reliable system to meet ITER requirements. ICH is likely to be a necessary component to advanced-mode plasma operation. Continuation of existing programs will keep the United States in the forefront of both the ICH and advanced-physics plasma operation modes. Future international collaborative programs with LHD, W7X, and STARX are also possibilities. Based on past experience, our collaboration with these programs would be welcomed and would allow cost-effective testing of U. S.-developed RF expertise on major fusion devices.

Electron Cyclotron Heating (ECH) Technology

ECH has many advantages for plasma heating and current drive applications. For heating, advantages of ECH include the well-known physics of the coupling of the wave to the plasma and the known location of the power deposition within the plasma. ECH can also be used for current drive and is especially useful for off-axis current drive. Such off-axis current drive is required in advanced tokamak scenarios. This would be an important application in the U.S. domestic fusion program in the future. ECH can be used for plasma stabilization and profile control, negative shear, etc. It is often used for heating stellarators, a major alternate concept. ECH has been selected as one of the main auxiliary heating methods for ITER. It will also be used for start-up and current drive.

ECH is in use at the DIII-D Tokamak at GA, T10 and T15 in Russia, Tore Supra in France, TCV in Switzerland, ASDEX-U in Germany, and JFT2M in Japan. It is in use on all major stellarators and helical devices such as W7AS in Germany, and LHD, CHS and Heliotron E in Japan. ECH is under development for ITER. The United States exports gyrotrons and transmission line components to Europe and Japan for use in these experiments.

The goal of the ECH technology development program is the development of 1 MW output power, continuous wave (CW) gyrotrons and the related systems components such as transmission lines and windows. Gyrotrons are needed at frequencies of 110 GHz for heating present day plasma devices, particularly the DIII-D tokamak at General Atomics, and at 170 GHz for ITER. ECH is a critical enabling technology for ITER, advanced tokamaks and alternate concepts such as stellarators. Maintaining a US capability, including industrial capability, in this enabling technology is extremely important and valuable.

The main development issue is demonstrating true CW operation at the 1 Megawatt power level, along with a transmission line and tokamak window. A second major issue is continuity of funding of industry. The US has a world renowned consortium of industry (CPI, formerly Varian), national labs, universities and small businesses in the ECH technology program. CPI has the only experience in the world with high average power / CW operation of a gyrotron. A CW power level of 300 kW has been achieved at 28 GHz and 200 kW at 140 GHz. Prototype development of a 110 GHz gyrotron has achieved over 100 kW of true CW operation and pulsed operation at 0.35 MW for 10 s, a record for gyrotron pulsed output energy. Extension of these results to the 1 MW, CW power level is very promising. The ITER prototype gyrotron under development at MIT has recently achieved 1 MW operation at 170 GHz in short pulse operation. This work validates the design and allows industrial development to go forward when funded.

ECH systems will be needed for ITER, DIII-D, Alcator C-Mod and other fusion devices throughout the world. Industrial participation is necessary for technology development and pays off for the United States in the long run since it allows sales abroad. Gyrotrons and transmission lines are possible components that the United States could contribute as a junior partner in ITER. This is especially true if we have the best product, which is the case at the present time. Without industrial participation, the United States could still contribute in the area of basic research. This is very unattractive but it would allow us to maintain our expertise in this important technological area and develop applications in the future.

Plasma Fueling Technology

Plasma fueling based on pellet injection is being pursued for a number of purposes:

- efficient fueling -- minimize tritium inventory and plasma exhaust throughput;
- plasma density profile control for improved reactivity and advanced confinement regimes (PEP-mode, shear reversal, etc.);

- rapid plasma termination for disruption mitigation - "killer pellets."

In the United States, pellet fueling R&D programs exist on DIII-D, TFTR, and C-MOD. In addition, the U.S. pellet injector development program plays an integrating role in the United States and world fusion programs by:

- collaborating on advanced pellet injector development with European partners;
- furnishing an improved centrifuge type fueling system for Tore Supra, and collaborating with the CEA on long pulse pellet fueling experiments;
- being the lead contributor to the design of the ITER fueling system.

The technologies developed in the U. S. fusion program have also been applied in areas outside of fusion research. These include the development of cryogenic pellet-based surface cleaning techniques and high Z pellet sources for advanced x-ray lithography systems. A viable base development program is essential for developing future spin-offs and to maintain our leadership role in fusion fueling research.

The long range objective is to develop reliable, steady state, tritium compatible plasma fueling systems to refuel fusion plasmas and control the fusion burn. An intermediate objective is to develop and supply pellet injection systems for use on magnetic confinement devices in the United States and abroad.

Issues which are currently being addressed for both long term and near term objectives are:

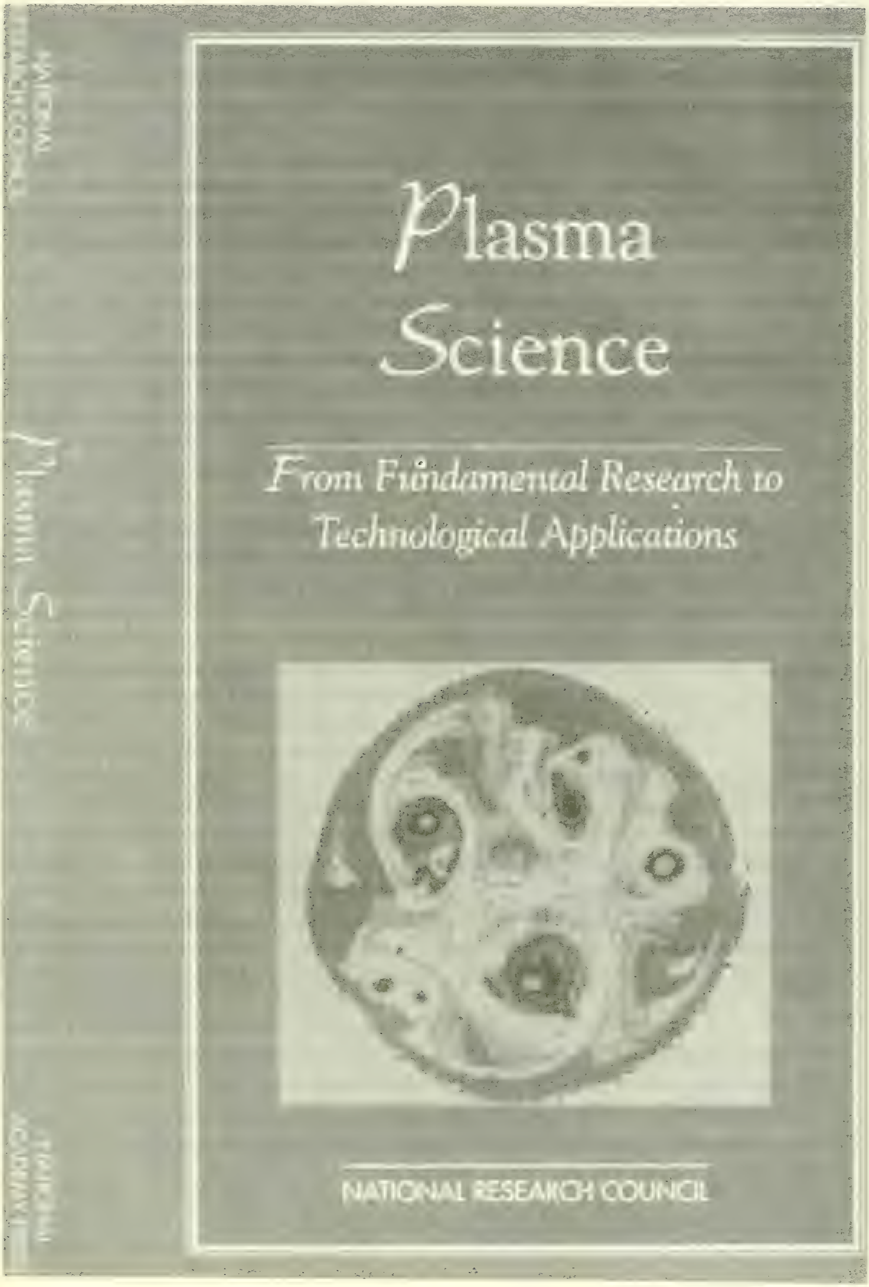
- the development of long pulse or steady state cryogenic pellet feed systems;
- tritium pellet fabrication and acceleration;
- reliable centrifuge and pneumatic drivers;
- higher pellet speeds (up to 4-5 km/s) for more demanding long term applications; and
- development of high Z pellet operating scenarios for disruption control studies.

The United States has the preeminent position in this field and is responsible for all of the R&D needs associated with ITER. Specific development expertise includes centrifuge and conventional pneumatic injectors, two-stage light gas guns and MHD railguns for higher performance, and tritium applications (unique). In addition, the U.S. program has a long history of applying fueling systems and conducting research on plasma confinement devices in the United States and abroad (enabling technologies).

Because of its unique position in this field, the United States can take the lead role in the design and construction of the ITER pellet fueling system. Expertise exists at U.S. laboratories and in industry to supply a cost-effective, reliable system to meet projected ITER needs. Potential future applications for this technology also include LHD (in Japan), and TEXTOR (in Germany). U.S.

collaboration on these programs is welcomed and would allow access to unique fusion facilities not planned in the domestic program .

Conclusion: *Plasma technology development is crucial to insure optimal advancement of plasma science and to establish fusion's feasibility as a viable energy source. The United States has developed unique expertise in these enabling technologies which can contribute to both domestic and international programs. Industry must continue to play a valuable role in plasma technologies.*



Plasma Science

Plasma

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This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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Cover: A snapshot of the electron density distribution in a magnetized, pure-electron plasma. These plasmas are nearly ideal, inviscid, two-dimensional fluids and are being used to study the relaxation and self-organization of fluid turbulence (see Plate 2 for details). (Courtesy of C.F. Driscoll, University of California, San Diego.)

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PANEL ON OPPORTUNITIES IN PLASMA SCIENCE AND TECHNOLOGY

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Preface

In the mid-1980s, the plasma physics volume of the series *Physics Through the 1990s* (National Research Council, National Academy Press, Washington, D.C., 1986) signaled problems for plasma science in the United States, particularly with regard to the basic aspects of the science. In the years that followed, there developed a widespread feeling in the plasma science community that something systematic needed to be done to address these issues. Out of this concern, the Plasma Science Committee of the Board on Physics and Astronomy was created in 1988. Following its establishment, plans were begun to undertake this study. With funding from the National Science Foundation, the Department of Energy, and the Office of Naval Research, the Panel on Opportunities in Plasma Science and Technology was appointed in May 1992, and the study began.

Approximately half of the 13-member panel consisted of experts in the many facets of plasma science considered in this report and half of scientists outside the field, with one of the co-chairs selected as a person with experience in science policy. Three of the members are from industry; one is from a government laboratory and one from an independent research society; and the remaining eight are from academe.

The task statement to the panel requested that this study examine virtually all aspects of plasma science and technology in the United States, assess the health of basic plasma science as a research enterprise, and identify and address key issues in the field. Specifically, the panel was charged with the task of conducting an assessment of plasma science that included beams, accelerators, and coherent radiation sources; single-species plasmas and atomic traps; basic plasma science in magnetic confinement and inertial fusion devices; space plasma physics; astrophysics; low-temperature plasmas; and theoretical and computational plasma science. It was directed to address the following:

1. Assess the health of basic plasma science in the United States as a research enterprise: (a) Identify and describe selected scientific opportunities. (b) Identify and describe selected technological opportunities. (c) Assess and prioritize new opportunities for research using the criteria of intellectual challenge, prospects for illumination of classic research questions, connection with other fields of science, and potential for applications. (d) Assess applications using the criteria of potential for contributing to industrial competitiveness, national defense, human health, and other aspects of human welfare.
2. Identify and address the issues in the field, including the following: (a) Evaluate the quality and size of the educational programs in plasma science in light of the nation's future needs. (b) Assess the institutional infrastructure in which plasma science is conducted, and identify changes that would improve the research and educational effort. (c) Characterize the basic experimental facilities needed to increase scientific productivity. (d) Develop a research strategy that is responsive to the issues. (e) Compare the U.S. program with those of Japan and Western Europe, and identify opportunities for international cooperation. (f) Identify the interactions and synergism with other areas of physics, chemistry, mathematics, and astronomy. (g) Assess the linkage of theory and experiment. (h) Assess manpower requirements and the prospects for meeting them. (i) Identify the users of plasma science and their needs.
3. Make recommendations to federal agencies and to the community that address these issues.

During the course of the study, the panel held three two-day meetings and two lengthy teleconferences. As part of the process, the panel took steps to solicit input from the plasma science community. Letters were sent to 200 scientists and engineers, requesting their input on the issues raised in the charge to the panel. This list was selected from the list of Fellows of the Plasma Physics Division of the American Physical Society (90), and it also included others suggested by members of the panel (65) and by grant officers involved in funding plasma science (45). The letters went to university faculty and staff (90), industrial scientists (25), staff at national laboratories (50), and others (5). A separate, more specialized survey was sent to 33 experimentalists engaged in basic plasma physics research. Input was also solicited by announcements of the panel's work that appeared in the newsletters of the American Geophysical Union, the American Physical Society, the Plasma Physics Division of the American Physical Society, the Committee on Plasma Science of the Institute of Electrical and Electronics Engineers (IEEE), and the University Fusion Associates. Town meetings were held at American Physical Society Plasma Physics Division meetings and the Gaseous Electronics Conference. There is general agreement from these sources on the themes expressed in this report: There is concern about the decline in basic plasma science, particularly in the area of basic plasma experimentation and other small-scale research efforts, and basic plasma science is perceived to lack a "home" in the federal agencies.

Also during the course of the study, the panel heard presentations from grant officers involved in funding plasma science from the Air Force Office of Scientific Research, the Advanced Research Projects Agency, the Department of Energy, the National Aeronautics and Space Administration, the National Science Foundation, and the Office of Naval Research.

The task statement requested that the panel assess specific areas of plasma science, such as beams, accelerators, and coherent radiation sources (called *topical areas* in the report), and *broad areas of plasma science*, including fundamental plasma experiments, theoretical and computational plasma physics, and education in plasma science. At the first meeting of the panel, these areas were renamed slightly and the topical area of low-temperature plasmas was added, since it had been omitted from the task statement through an oversight. The resulting seven topical areas are assessed in Part II of the report, and the three broad areas of plasma science are assessed in Part III. Part IV consists of some concluding remarks.

During the course of the study, the panel had numerous discussions about the desirability of establishing organizational units specifically devoted to plasma science in the relevant federal agencies. Many members of the plasma science community who were consulted strongly advocated the establishment of such homes, believing that they are needed if basic plasma science is to be given the focused attention and increased support that the panel recommends. While this subject is beyond the scope of the panel's work, the panel suggests that the federal government might give this issue further consideration.

The National Academy of Sciences is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Bruce Alberts is president of the National Academy of Sciences.

The National Academy of Engineering was established in 1964, under the charter of the National Academy of Sciences, as a parallel organization of outstanding engineers. It is autonomous in its administration and in the selection of its members, sharing with the National Academy of Sciences the responsibility for advising the federal government. The National Academy of Engineering also sponsors engineering programs aimed at meeting national needs, encourages education and research, and recognizes the superior achievements of engineers. Dr. Robert M. White is president of the National Academy of Engineering.

The Institute of Medicine was established in 1970 by the National Academy of Sciences to secure the services of eminent members of appropriate professions in the examination of policy matters pertaining to the health of the public. The Institute acts under the responsibility given to the National Academy of Sciences by its congressional charter to be an advisor to the federal government and, upon its own initiative, to identify issues of medical care, research, and education. Dr. Kenneth I. Shine is president of the Institute of Medicine.

The National Research Council was established by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and of advising the federal government. Functioning in accordance with general policies determined by the Academy, the Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in providing services to the government, the public, and the scientific and engineering communities. The Council is administered jointly by both Academies and the Institute of Medicine. Dr. Bruce Alberts and Dr. Robert M. White are chairman and vice chairman, respectively, of the National Research Council.

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In preparing this report, the Panel on Opportunities in Plasma Science and Technology has benefited greatly from the assistance of many members of the plasma science community. We are particularly indebted to the former chairs of the Plasma Science Committee of the Board on Physics and Astronomy, C.F. Kennel and F.W. Perkins, and the present chair, Ravi Sudan, for their advice and help. The other members of the Plasma Science Committee also provided valuable advice during the course of the study.

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Plasma Science

From Fundamental Research to Technological Applications

Executive Summary

Plasma science is the study of the ionized states of matter. Most of the observable matter in the universe is in the plasma state. Plasma science includes plasma physics but aims to describe a much wider class of phenomena in which, for example, atomic and molecular excitation and ionization processes and chemical reactions can play significant roles. The intellectual challenge in plasma science is to develop principles for understanding the complex macroscopic behavior of plasmas, given the known principles that govern their microscopic behavior.

Plasmas of interest range over tens of orders of magnitude in density and temperature—from the tenuous plasmas of interstellar space to the ultradense plasmas created in inertial confinement fusion, and from the cool, chemical plasmas used in the plasma processing of semiconductors to the thermonuclear plasmas created in magnetic confinement fusion devices. A healthy plasma science enterprise can be expected to make many important contributions to our society for the foreseeable future. The purpose of this report is to provide guidance regarding the ways in which plasma science can contribute to society and to recommend actions that will optimize these contributions.

FINDINGS

1. Plasma science impacts daily life in many significant ways. It plays an important role in plasma processing, the sterilization of medical products, lighting, and lasers. Plasma science is central to the development of fusion as an

energy source, high-power radiation sources, intense particle beams, and many aspects of space science.

2. Plasma science is a fundamental scientific discipline, similar, for example, to condensed-matter physics. This fact is apparent when one considers the commonality of the intellectual problems in plasma science that span the wide range of applications to science and technology. Despite its fundamental character, plasma science is frequently viewed in the academic community as an interdisciplinary enterprise focused on a large collection of applications. Experiment, theory, and computation are all critical components of modern plasma science.

3. While the applications of plasma science have been supported by the federal government, no agency has assumed responsibility for basic research in plasma science. In general, there is a lack of coordination of plasma science research among the federal agencies.

4. As the development of plasma applications has progressed, small-scale research efforts have declined, particularly in the area of basic plasma experiments. This decline has led to a significant backlog of important scientific opportunities. This core activity in fundamental plasma science, carried out by small groups and funded by principal-investigator grants, is dangerously small, considering its importance to the national effort in fusion energy and other applied programs.

5. Plasma scientists in academic institutions are less likely to be in tenure-track positions than are other physicists, and courses in plasma science are currently unavailable at many educational institutions.

CONCLUSIONS

1. Plasma science can have a significant impact on many disciplines and technologies, including those directly linked to industrial growth. To properly pursue the potential offered by plasma science, the United States must create and maintain a coherent and coordinated program of research and technological development in plasma science.

2. Recognition as a distinct discipline in educational and research institutions will be crucial to the healthy development of plasma science.

3. There is no effective structure in place to develop the basic science that underlies the many applications of plasmas, and if the present trend continues, plasma science education and basic plasma science research are likely to decrease both in quality and quantity. If nothing is done by the federal government, it is likely that research in basic plasma science will cease to exist, and progress in the applications that depend on it will eventually halt.

4. The future health of plasma science, and hence its ability to contribute to the nation's technological development, hinges on the revitalization of basic plasma science and, in particular, on the revitalization of small-scale basic plas-

ma experiments. With regard to theory and modeling, although the current programs have been successful, there is a need for individual-investigator-led research on questions fundamental to basic plasma science.

5. Coordination of research efforts is vital, to make the most effective use of resources by maintaining complementary programs and to ensure that all critical problems are addressed.

6. Because of the commonality underlying all areas of plasma science, renewed emphasis on basic plasma science will benefit all areas. Therefore, it is appropriate that redistribution of funding to support basic plasma science come from all areas of plasma science.

RECOMMENDATIONS

1. To reinvigorate basic plasma science in the most efficient and cost-effective way, emphasis should be placed on university-scale research programs.

2. To ensure the continued availability of the basic knowledge that is needed for the development of applications, the National Science Foundation should provide increased support for basic plasma science.

3. To aid the development of fusion and other energy-related programs now supported by the Department of Energy, the Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, should provide increased support for basic experimental plasma science. Such emphasis would leverage the DOE's present investment in plasma science and would strengthen investigations in other energy-related areas of plasma science and technology.

4. Approximately $15 million per year for university-scale experiments should be provided, and continued in future years, to effectively redress the current lack of support for fundamental plasma science, which is a central concern of this report. Furthermore, individual-investigator and small-group research, including theory and modeling as well as experiments, needs special help, and small amounts of funding could be life-saving. Funding for these activities should come from existing programs that depend on plasma science. A reassessment of the relative allocation of funds between larger, focused research programs and individual-investigator and small-group activities should be undertaken.

5. The agencies supporting plasma science should cooperate to coordinate plasma science policy and funding.

6. Members of the plasma community in industry and academe should work aggressively for tenure-track recognition of plasma science as an academic discipline, and work with university faculty and administrators to provide courses in basic plasma science at the senior undergraduate level.

Additional recommendations regarding specific areas of plasma science are made in the main text of the report.

PART I

◆

Overview

INTRODUCTION

Plasma science is the study of the ionized states of matter. Plasmas occur quite naturally whenever ordinary matter is heated to a temperature greater than about 10,000°C. The resulting plasmas are electrically charged gases or fluids. They are profoundly influenced by the long-range Coulomb interactions of the ions and electrons and by the presence of magnetic fields, either applied externally or generated by current flows within the plasma. The dynamics of such systems are complex, and understanding them requires new concepts and techniques.

Plasma science includes plasma physics but aims to describe a much wider class of ionized matter in which, for example, atomic, molecular, radiation-transport, excitation, and ionization processes, as well as chemical reactions, can play significant roles. Important physical situations include partially ionized media and the interaction of plasmas with material walls. Thus plasma science draws on knowledge and techniques from many areas of science, including chemistry, fluid dynamics, and large-scale numerical computation, to achieve an accurate description of plasma behavior.

The goal of *plasma physics* is to describe elementary processes in completely ionized matter. In common with such fields as chemistry, condensed matter physics, and molecular biology, plasma physics is founded on well-known principles at the microscopic level. Description of plasmas typically involves use of Maxwell's equations for the electromagnetic fields and the Liouville or Boltzmann equations to model the dynamics of the electrons and ions, which are treated as point charges. Simpler approximations based on fluid descriptions for the electrons and ions (e.g., magnetohydrodynamics) are also used. The plasma medium is inherently nonlinear because the charged particles composing the plasma interact collectively with the electromagnetic fields produced self-consistently by the charge density and currents associated with the plasma particles.

Much of the basis for analyzing and treating plasmas has now been laid out, and a number of important advances in our understanding have been made. However, we are far from being able to make quantitative predictions of plasma behavior in many, if not most, of its manifestations. The intellectual challenge in plasma physics is to develop principles for understanding the complex macroscopic behavior of plasmas, given the known principles that govern their microscopic behavior.

The development of plasma science in the past three decades has been propelled by applications such as fusion energy, space science, and the need for a strong national defense, and this support has resulted in significant progress. Yet, by necessity, only those aspects that appeared to be more or less directly pertinent to applications received the lion's share of attention. Plasma science has benefited greatly from this support, but the field has now reached a level of maturity where many basic issues have been identified and remain to be resolved. Further progress will depend eventually on addressing these basic issues, rather than focusing only on the demands made on plasma science by applications. In turn, a greater understanding of the fundamentals of plasma science can be expected to advance significantly its successful application to the needs of society. Progress will be greatly inhibited without a strong experimental and theoretical research program directed at the fundamental principles of plasma science and not constrained to focus only on near-term applications. In particular, although theoretical and computational studies have spearheaded many of the advances in plasma physics in the past, well-planned and precisely controlled experiments will be crucial to further progress.

The panel was charged with the assessment of specific areas of plasma science that it refers to as *topical areas.* These include low-temperature plasmas, nonneutral plasmas, inertial and magnetic confinement fusion, beams, accelerators, and coherent radiation sources, and space and astrophysical plasmas. These areas vary in size, the nature of the scientific efforts, and the key scientific and organizational challenges facing them. Part II contains assessments of these topical areas with conclusions and recommendations specific to each. The panel was also charged with the assessment of *broad areas of plasma science:* basic plasma experiment, theory and computational plasma physics, and plasma science education; this is done in Part III. Although research and development in the topical areas is proceeding reasonably well, the panel's conclusion is that maintaining the vitality of basic plasma science faces severe difficulties unless there is concerted action by both the funding agencies and the scientific community. Because of the importance of present and potential applications of plasma science to our society, much benefit would be gained by a coherent program of support for basic plasma science. Much of the remainder of this overview chapter is devoted specifically to this issue, and the chapter concludes with a summary of the central messages of the report and the panel's general conclusions and recommendations.

THE ROLE OF PLASMA SCIENCE IN OUR SOCIETY

Plasma science impacts daily life in many significant ways. Low-temperature plasmas, in which electric fields in the plasma can impart significant energy to the electrons and ions but the plasmas are still cool enough to support a multitude of chemical reactions, are critical to the processing of many modern materials. This method of "plasma processing" is an enabling technology in the fabrication of semiconductors. Important applications include the plasma etching of semiconductors and the surface modification and growth of new materials. A recent National Research Council report,[1] which highlights the importance of plasma processing in the electronics industry, indicates that the worldwide sale of plasma reactors alone amounted to $1 billion dollars in 1990 and is expected to double in the next five years. Other important uses of low-temperature plasmas include the "cold" pasteurization of foods, the sterilization of medical products, environmental cleanup, gas discharges for lighting and lasers, isotope separation, switching and welding technology, and plasma-based space propulsion systems.

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[1] National Research Council, *Plasma Processing of Materials: Scientific Opportunities and Technological Challenges,* National Academy Press, Washington, D.C., 1991.

Coherent radiation sources and particle accelerators rely on plasma concepts. Uses of intense electron beams include the bulk sterilization of medical products and food, toxic waste destruction via oxidation, processing of advanced materials, and new welding techniques. Free-electron-laser radiation sources have a variety of potential applications in medicine and industry, and they offer the possibility of intense, tunable sources of electromagnetic radiation in virtually all parts of the electromagnetic spectrum. Nonneutral plasmas in electromagnetic traps have application as ultraprecise atomic clocks and as a method to confine and manipulate antimatter such as positrons and antiprotons.

Plasma science is central to the development of fusion as a clean, renewable energy source. In order to control the fusion process, which is the source of energy of the Sun and the stars, we must learn to create hot, dense plasmas of deuterium and tritium in the laboratory. Great progress has been made toward this goal. Fusion-plasma confinement times have increased by a factor of more than 100 in the last two decades, and achievable temperatures have increased by a factor of 10. There is now in place an international collaboration to design the first prototype fusion power reactor, the International Thermonuclear Experimental Reactor (ITER). However, the continued refinement of the fusion concept and the optimization of fusion as a power source will require improved understanding of methods of confining and heating plasmas, as well as the development of techniques to lessen the damage to material walls due to the close proximity of the fusion-temperature plasmas. The leverage on investment in this area is tremendous. All major industrial nations have experienced a steady increase in the use of electricity—it is the energy type of choice. Nuclear fission plants are aging, fossil fuels continue to be of concern due to the production of greenhouse gases, and fusion offers the potential of large-scale electricity generation with abundant fuel supply and attractive environmental features.

We live in the 1% or so of the universe in which matter is not ionized, so plasmas are not readily apparent in our daily lives. However, as illustrated in Figure S.1, plasmas occur in many contexts, spanning an incredible range of plasma densities and temperatures. The most common examples of plasmas that we can actually see are the gas discharges in neon lights and the discharges in bolts of lightning. Most of the observable matter in the universe is in the plasma state (i.e., in the form of positively charged ions and negatively charged electrons). Plasma science provides one of the cornerstones of our knowledge of the Sun, the stars, the interstellar medium, and galaxies. We cannot understand such

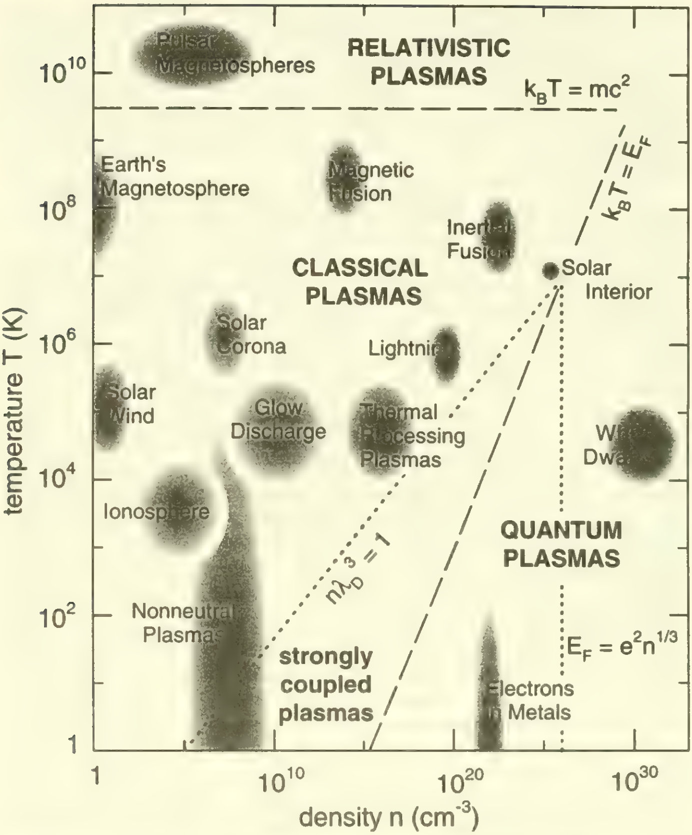

FIGURE S.1 Plasmas that occur naturally or can be created in the laboratory are shown as a function of density (in particles per cubic centimeter) and temperature (in kelvin). The boundaries are approximate and indicate typical ranges of plasma parameters. Distinct plasma regimes are indicated. For thermal energies greater than that of the rest mass of the electron ($k_BT > mc^2$), relativistic effects are important. At high densities, where the Fermi energy is greater than the thermal energy ($E_F > k_BT$), quantum effects are dominant. In strongly coupled plasmas (i.e., $n\lambda_D{}^3 < 1$, where λ_D is the Debye screening length), the effects of the Coulomb interaction dominate thermal effects; and when $E_f > e^2n^{1/3}$, quantum effects dominate those due to the Coulomb interaction, resulting in nearly ideal quantum plasmas. At temperatures less than about 10^5 K, recombination of electrons and ions can be significant, and the plasmas are often only partially ionized.

phenomena as sunspots, the formation of stars from interstellar gas clouds, the acceleration of cosmic rays, the formation and dynamics of energetic jets and winds from stars and galaxies, or the interaction of supernova remnants with interstellar gas, without the concepts of plasma science. Plasmas are central to many aspects of space science. The space plasma medium extends from the ionosphere surrounding the Earth to the far reaches of the solar system. "Space-weather" prediction in the ionosphere and magnetosphere is important for global communications, and the properties of space plasmas are important in determining the capabilities and longevity of spacecraft. Thus, although it is often not readily apparent, plasma science affects our society in a myriad of ways.

THE DISCIPLINE OF PLASMA SCIENCE

Common Research Themes

The panel has concluded that plasma science is frequently viewed, not as a distinct discipline, but as an interdisciplinary enterprise focused on a large collection of applications. The underlying, common, and critical feature of plasma science as a discipline is that its goal is to understand the behavior of ionized gases, and this requires fundamentally different techniques from those applicable to uncharged gases, fluids, and solids. This coherence of plasma science as a discipline is apparent when one considers some of the challenging intellectual problems, central to plasma science, that span applications in many of the topical areas. The impact of four such problems on the topical areas assessed in this report are summarized in Table S.1.

Wave-Particle Interactions and Plasma Heating

Understanding the interaction of plasma particles with the collective plasma oscillations and waves is a fundamental question with many practical applications. Basic scientific issues involve the trapping of particles in waves, the nonlinear saturation of wave damping, chaotic behavior induced in particle orbits, and particle acceleration mechanisms. Wave heating is an important method of heating fusion plasmas to the required temperatures for fusion. Waves can be used to drive electrical currents in plasmas. One promising scheme for a steady-state tokamak fusion reactor is to use waves to drive electrical currents to confine the plasma, instead of the present method of driving pulsed currents inductively. Wave-particle interactions are central to the operation of free-electron lasers and other coherent radiation sources, to many advanced accelerator concepts, and to methods of creating and heating low-temperature plasmas for plasma processing applications. Wave-particle processes are important in Earth's magnetosphere and ionosphere, and shock waves are the dominant production mechanism for cosmic rays of astrophysical origin.

TABLE S.1 Applications of Basic Plasma Research, Illustrating the Commonality of Scientific Issues Across Topical Areas

	Scientific Issue			
Topical Area	Wave-Particle Interactions and Plasma Heating	Chaos, Turbulence, and Transport	Sheaths and Boundary Layers	Magnetic Reconnection and Dynamos
Low-temperature plasmas	Magnetrons Plasma sprays	Instabilities in plasma processing	Plasma processing Lighting	Plasma torches MHD drag reduction
Nonneutral plasmas	ICR mass spectrometry	Precision clocks Fluid flows Antimatter storage	Switches Diodes	—
Inertial confinement fusion	Parametric instabilities Preheating	Turbulent mixing Rayleigh-Taylor instability	Plasma-driver interface	ICF plasma magnetic fields
Magnetic confinement fusion	rf current drive rf plasma heating	Energy and particle loss	Divertor operation Plasma-limiter interaction	Sawteeth and current profile dynamics in tokamaks Reversed-field pinches

Beams, accelerators, and coherent radiation sources	FELs Advanced accelerators	Instabilities in FELs and advanced accelerators	Cathodes	—
Space plasmas	Magnetosphere Ionosphere	Cometary and planetary atmospheres Solar wind	Magnetospheric and ionospheric boundaries Plasma-satellite interaction	Solar interior and corona Magnetopause
Astrophysical plasmas	Cosmic-ray acceleration	Accretion disks Dynamo viscosity	Pulsar magnetospheres Accretion disk boundaries	Solar and stellar magnetic fields

NOTE: ICR = ion cyclotron resonance
rf = radio-frequency
FEL = free-electron laser
MHD = magnetohydrodynamic
ICF = inertial confinement fusion

Chaos, Turbulence, and Transport

Most plasmas of interest are nonuniform in density and temperature, which results in the excitation of turbulent waves and fluctuations. These fluctuations in turn produce the transport of particles and energy that tend to drive the plasma toward a more uniform state. Very generally, turbulence and turbulent transport are not understood. The recent renaissance in nonlinear dynamics and studies of phenomena such as chaos provide new tools with which to attack these problems, and in fact, plasmas offer a convenient and often unique medium in which to study turbulent phenomena. Many of the scientific issues are now clear, and the plasma applications are many. For example, turbulent transport is the dominant mechanism for energy and particle transport in tokamak fusion plasmas. Turbulent transport is frequently the dominant particle and energy loss mechanism in low-temperature plasmas. It is important in the Earth's magnetosphere, in stellar convection zones, and in astrophysics in settings such as the interstellar medium.

Plasma Sheaths and Boundary Layers

Understanding the boundaries of plasmas (called sheaths) is a well-defined problem with many practical consequences. In magnetically confined fusion plasmas, the hot plasma cannot be allowed to contact the material walls. The result is that there must be large gradients in plasma temperature and density in the plasma and, frequently, non-equilibrium particle distributions. The precise character of these boundary layers can greatly influence the character of the bulk plasma and the rate at which wall damage occurs. Plasma sheaths are important in the plasma processing of materials. This sheath is adjacent to the material surface to be processed; therefore, the properties of this layer determine the characteristics of the plasma-matter interaction. A similar phenomenon occurs in space plasmas, where a plasma sheath separates a satellite from the surrounding space plasma, and the properties of this sheath determine the interaction of the plasma particles with the satellite. This is relevant for considerations such as surface damage and electrical phenomena. A type of boundary layer called a "double layer" can separate regions of plasma with distinctly different properties. Double layers are known to occur both in laboratory and in space plasmas, where they play an important role in determining the global configuration of the plasma. Related electrode-sheath phenomena are the least well understood aspects of lighting plasmas, and learning to control them better would lead to more efficient and longer-life products.

Magnetic Reconnection and Dynamo Action

The behavior of a magnetized plasma is determined largely by the configuration of the magnetic field in the plasma. Currents and plasma flows can induce changes in the topology of the field by breaking and reconnecting the magnetic field lines. This process occurs in (magnetohydrodynamic) "sawtooth" oscillations in tokamak plasmas, in sun spots, and in many astrophysical plasmas.

Dynamo action is the process by which a flowing plasma converts mechanical energy into magnetic field energy. This process is thought to be the origin of Earth's magnetic field, and it is likely to be one of the mechanisms for producing astrophysical magnetic fields. Very little is understood about magnetic reconnection and dynamo action, yet new techniques are now available to address these problems, by analytic methods, by computer simulation, and by suitably designed laboratory experiments.

These and other forefront problems in plasma science are described in Part III, and the relationship of this research to specific topical areas and applications is discussed in Part II.

Research and Education in Plasma Science

The findings and conclusions regarding the three broad areas of plasma science assessed in Part III are discussed in this section.

Basic Plasma Experiments

Of any of the topics in the panel's study, basic plasma experiments constitute the area of greatest concern. Progress in the physical sciences has relied historically on the close interplay between theory and experiment. Perhaps nowhere is this more true than in many-body physics, which naturally includes plasma physics. Physical phenomena can be identified, isolated, and studied most efficiently, quickly, and economically in experiments specifically tailored for this purpose. There are many advantages of basic experiments, compared to experiments done in settings determined by other considerations such as particular applications. These advantages include the flexibility to choose the setting to isolate a particular physical phenomenon, the ability to explore the broadest range of plasma parameters, and the ability to make experimental changes quickly, guided by the internal logic of the underlying science and by new results as they unfold.

Despite the importance of basic plasma experiments to plasma science, there have been clear warning signs for more than a decade of a deficiency in this area. This was expressed clearly in the Brinkman report, *Physics Through the 1990s*, written almost a decade ago.[2] The finding of the panel is that this situation has worsened since the Brinkman report was issued. There are several causes of this problem, which include the narrowing focus of the large applied programs such as fusion and space applications. The future health of plasma science as a discipline hinges on the revitalization of basic plasma science, particularly the revitalization of small-scale basic plasma experiments—the area of most rapid decline in the last 20 years. Plasma science is suffering from application without replenishment: With major emphasis on applying what is known and without maintaining the basic scientific effort, the "seed corn" is quickly disappearing.

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[2] National Research Council, *Plasmas and Fluids*, in the series *Physics Through the 1990s*, National Academy Press, Washington, D.C., 1986.

Although support for basic plasma science has declined over the past two decades, there has been important progress, which provides an idea of the potential contributions that basic research can make. Important achievements include a deeper understanding of the interaction of plasma waves with plasma particles. Many important nonlinear plasma processes have been isolated and understood, including some aspects of double layers, which, as discussed above, are the nonlinear interfaces between regions of plasma with distinctly different plasma properties; the effects of ponderomotive forces in causing the reorganization of plasmas; the filamentation of electromagnetic radiation in plasmas; and some aspects of the reconnection of magnetic field lines and magnetic reconfiguration of plasmas. Recently, the effects of chaotic particle orbits on plasma behavior have begun to be addressed by laboratory experiments. Each of these phenomena has many potential applications, so that progress on one topic is likely to have an impact on several different areas of plasma science. Progress has also been made in developing new techniques for experimental plasma studies, including some developed specifically for plasma applications and others that have resulted from advances in technologies such as electronics and computing.

As the development of applications has progressed, the decline of basic plasma experiments over the past 20 years has led to a significant backlog of important opportunities for basic experiments. We lack a basic understanding of many aspects of Alfvén waves, relevant to both space and fusion plasmas. We lack a basic understanding of magnetic reconfiguration processes and a host of other important nonlinear phenomena, including the wave-plasma interactions, plasma sheaths and boundary layers in magnetized plasmas, and dynamo action, discussed above. Progress in understanding each of these phenomena has been hindered by the lack of basic experiments designed to address key issues.

To reinvigorate experimental plasma science in the most efficient and cost-effective way, the panel concludes that emphasis should be placed on support for university-scale research programs. This conclusion is based on two findings: Many if not most of the important outstanding problems in basic plasma science can be addressed by this mode of experimentation. In addition, where small and individual principal-investigator-led programs are possible, the degree of flexibility, diversity, and creativity associated with this mode of research optimizes the limited resources that can be expected to be available for basic plasma research in the foreseeable future.

The panel estimates the size of the investment that will be required in the following way. Healthy, small-scale experimental research programs in plasma science typically involve a senior researcher, a postdoctoral researcher, and three to six graduate students. In addition, due to the nature of plasma experiments, an electronics technician is frequently required. The cost of such a group, including equipment purchases, typically ranges from $200,000 to $400,000 per year. (See Chapter 8 for details.) Given the erosion of the experimental infrastructure in basic plasma science, typical initial equipment purchases of from $300,000 to $600,000 are required for each new program. Based on the decline of basic experimental plasma science over the past 20 years, the panel estimates that a total of 30 to 40 new programs will be required to revitalize this area. Thus, the cost of such an effort would be approximately $15 million per year.

The panel's survey of the experimental research community indicates that the present number of efforts in the United States of the type described above, basic experimental plasma research, is less than 20. The required number of new groups (30 to 40) was estimated by considering the minimum-size scientific community that would provide sufficient cross-fertilization of ideas and stimulation, spread over this broad area of basic plasma science. Consideration of the size of a research community necessary to cover the existing range of forefront scientific problems (discussed in Part III) gives a similar estimate.

Theory and Computational Plasma Physics

Complementing experimental observation, theory, and computation are critical components of modern plasma science. The theoretical problems in plasma science are formidable. The goal is to achieve quantitative understanding of nonlinear, many-body phenomena in these nonequilibrium systems. Much progress has been made in this area in the past two decades, particularly in applied areas where there have been concentrations of effort, such as fusion, space plasmas, and plasmas relevant to defense applications. Examples include magnetohydrodynamic phenomena relevant to tokamak fusion plasmas, where the stability of these plasmas and the macroscopic dynamics of unstable plasmas are now quantitatively understood. Beyond such fluid models of plasma behavior, progress has been made in more detailed and powerful statistical mechanical descriptions of plasmas, including the effects of large particle orbits on plasma stability and the so-called gyrokinetic model of plasma dynamics. Many other nonlinear plasma problems have been addressed successfully, including the dynamics that results when an intense beam of electromagnetic radiation is incident upon a non-uniform plasma.

Fundamental understanding of a variety of coherent structures has been achieved—vortices and nonlinear phase-space correlations, for example. These concepts are of considerable significance because they have the potential to lead to simplified descriptions of otherwise complicated plasma behavior. Numerical simulations have provided deeper insights into a variety of plasma processes.

This progress has been driven by advances in computational techniques in general and also by the invention of new algorithms designed specifically for plasma computations.

Future challenges to theoretical and computational plasma physics arise from several different considerations. As mentioned above, the description of plasma behavior, given that plasmas are nonlinear and nonequilibrium many-body systems, typically presents the theorist with very difficult problems. In addition, a wide variety of parameter ranges and boundary conditions are relevant to plasmas of interest. Finally, the fact that there is a vast range of practical applications of plasmas—from plasma processing, to new radiation sources and accelerators, to fusion and global communications—requires not just a qualitative but a quantitative understanding of the underlying plasma behavior.

One particularly important and broad topic in theoretical and computational plasma science is the achievement of a deeper understanding of plasma turbulence and the associated transport that frequently results from it. Another broad area of fundamental importance is understanding the evolution of currents, flows, and magnetic fields in plasmas. These problems, which typically involve a breaking of the spatial symmetry, can lead to coherent structures and multiple length scales that are difficult to treat theoretically or numerically. Yet such phenomena are important in applications ranging from low-temperature plasmas for materials processing and welding to space and astrophysical plasmas.

While the current programs of theory and modeling have been successful, particularly in the context of the large applied programs, the panel concludes that two modes of research need to be reemphasized. There is a need for individual-investigator-led research on questions fundamental to basic plasma science, such as stochastic effects, novel analytical techniques, and a variety of nonlinear processes. Emphasis also needs to be put on pursuing the commonality of physical processes and mathematical techniques across the many subdisciplines of plasma science.

Education in Plasma Science

Plasma science is a fundamental scientific discipline that has made and continues to make significant contributions to our society. The field is intrinsically interdisciplinary, much like modern materials science. Yet, there needs to be a focus and home for plasma science in modern educational and research institutions for the field to develop properly and have the maximum impact. The panel has found that plasma scientists are less likely to be in tenure-track positions than are other physicists. Presently, courses in plasma science are unavailable at many educational institutions. If this trend continues, plasma science education and basic plasma science research are likely to decrease in both quality and quantity.

The entire field of plasma science, including its educational function, will be strengthened by the panel's recommendation that increased support of basic plasma science and university-scale research be provided by the National Science Foundation and the Department of Energy. The programs supported in this way can be expected to attract and maintain the scientific talent necessary to provide high-quality educational programs in plasma science and technology.

To develop adequate educational programs, given the breadth and diversity of plasma science, the panel makes the following recommendations to the plasma community. Since plasma science can contribute to a wide variety of scientific disciplines, educational programs should include courses tailored to the needs of areas such as low-temperature plasma physics and plasma chemistry, plasma processing, and astrophysical plasma physics, in addition to the major programs of fusion and space plasma physics. The panel believes that these programs would be helped by textbooks with the particular needs of these areas and relevant applications in mind. The panel suggests that senior-level courses in plasma physics become standard offerings in undergraduate curricula and that chapters on plasma physics be developed for more general science textbooks, to increase the level of plasma literacy of scientists and engineers outside the field.

SUMMARY OF TOPICAL AREAS

As described in the preface, the panel was charged with assessing specific topical areas of plasma science: low-temperature plasmas; nonneutral plasmas; inertial and magnetic confinement fusion; beams, accelerators, and coherent radiation sources; and space and astrophysical plasmas. This section contains an overview of these assessments.

Low-Temperature Plasmas

Low-temperature plasmas include those with many important technological applications, such as the plasma processing of materials for electronics, "cold" pasteurization of foods and sterilization of medical products, environmental cleanup, gas discharges for lighting and lasers, isotope separation, switching and welding technology, and plasma-based space propulsion systems. As an example of the impact of these technologies on industry and business, the use of plasma processing of semiconductors for electronics was recently reviewed by a separate National Research Council study.[3] The annual sales in equipment for the plasma processing of semiconductors amounted to $1 billion per year in 1990 and are projected to grow to $2 billion in 1995.

Low-temperature plasma science requires an understanding of atomic and molecular physics, plasma chemistry, and plasma physics. Many fundamental low-temperature plasma phenomena are poorly understood. Yet modern techniques are available to address a range of problems of fundamental importance that have important practical applications. Examples include the physics and chemistry of plasmas at material boundaries (i.e., plasma "sheaths"), the creation of plasmas by electrodeless discharges, and the stability and reproducibility of plasma discharges.

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[3] National Research Council, ***Plasma Processing of Materials: Scientific Opportunities and Technological Challenges***, National Academy Press, Washington, D.C., 1991.

There are many important applications of low-temperature plasma science, yet there is no structure in place to support the fundamental research in this area that will be required to systematically develop these applications. Federal agencies have traditionally had only modest efforts in low-temperature plasma research; and recently, they have deemphasized these programs. Industry has usually engaged only in projects for which there is a short-term payoff. In contrast, there is an active effort in this area in Japan, and a large effort in low-temperature plasma research has recently been created in France. The crucial problem regarding low-temperature plasma science in the United States is the lack of a coordinated governmental program in this technologically important area. The panel recommends the creation of a coordinated support structure for fundamental research in low-temperature plasma science.

Nonneutral Plasmas

Nonneutral plasmas include pure electron plasmas and pure ion plasmas in electromagnetic and electrostatic traps, electron beams, and ion beams. Examples of applications of nonneutral plasmas are electron beams and plasmas for the generation of electromagnetic waves, pure ion plasmas in traps for atomic clock applications, advanced concepts for particle accelerators, and the confinement of antimatter such as positrons and antiprotons.

Nonneutral plasmas are more easily confined than neutral plasmas. Consequently, they can be more easily controlled and studied. Important questions that have recently been addressed include issues of plasma confinement, the creation of thermodynamic equilibrium states and controlled departures from equilibrium, and the mechanisms for the transport of particles and energy. Many of the concepts developed in the study of nonneutral plasmas have wider applications to understanding the physics of neutral plasmas and to fluid dynamics and atomic physics.

Nonneutral plasma physics is one area of basic plasma science that has progressed dramatically in the past two decades, and questions of fundamental importance have been addressed that are also relevant to technological applications. This was due, in large part, to a program of dedicated support for research in this area by the Office of Naval Research. This successful support of experimental and theoretical research on nonneutral plasmas should be used as a model for a program of renewed support of basic experiments in neutral plasmas that is recommended elsewhere in this report.

Nonneutral plasma research will be an important area for the foreseeable future, both from the point of view of fundamental plasma science in neutral and nonneutral plasmas and in exploiting this knowledge for technological applications. The panel recommends that continued support be given to research in this area and to the development of technological applications.

Inertial Confinement Fusion

The goal of inertial confinement fusion is to harness fusion power using intense lasers or ion beams to compress fusionable material, such as deuterium and tritium. The required plasma parameters for inertial confinement fusion are densities as much as 100 times that of ordinary matter and temperatures in excess of 100 million Kelvin. Much progress has been made in this area in the last decade. Important, new diagnostic techniques have been developed. The largest laser project, Nova, has conducted experiments to study the important problem of the physics of interpenetrating materials at accelerated interfaces. Computer simulations have clarified the role that fluid dynamical instabilities play in the dynamics of target compression. However, many challenging problems remain to be addressed. Examples include understanding stimulated Raman and Brillouin instabilities in laser-plasma interactions, particularly at high laser intensities, and understanding nonlinear plasma instabilities and the equations of state and opacity of matter at high densities and temperatures.

Many of the outstanding problems in this area have a high degree of commonality with important problems in other areas of science. Examples relevant to space physics and fusion include questions of plasma turbulence, particle acceleration and heating by electromagnetic radiation, and the effects of spatial inhomogeneities on wave propagation and mode conversion. Other important problems have much in common with optical science. Research in inertial confinement fusion can also benefit other fields. Examples include the development of short-pulse and x-ray lasers.

While progress toward inertial confinement fusion has been good, continued emphasis on programmatic milestones could leave unaddressed fundamental scientific questions crucial to the achievement of future goals. It is important for the program to reemphasize a broad-based program of support for relevant areas of basic research.

The commonality of scientific problems with other areas of science could be used to facilitate progress, both in the inertial fusion program and in related areas. Much of the fusion target program in the United States had been classified for security reasons. Much benefit should be gained by declassification currently being done by the Department of Energy. Given the commonality of problems in this area with those in other areas of plasma science and the importance of basic research in related fields to the achievement of fusion, the panel recommends that some resources be reallocated within the program to support the study of basic science relevant to inertial confinement fusion. Further, the panel recommends that the use of inertial confinement fusion facilities by scientists, working outside the program but on relevant problems, be encouraged.

Magnetic Confinement Fusion

Magnetic confinement fusion continues to be the largest driver for the intellectual development of plasma science. Central to the achievement of fusion in magnetically confined plasmas is the ability to confine hot plasmas (i.e., those with temperatures of more than 100 million Kelvin). Since these plasmas must eventually come in contact with material boundaries, this program also involves important considerations concerning low-temperature plasma science.

Much progress has been made in this field over the past two decades. Confinement times of fusion plasmas have increased by a factor of more than 100, and achievable temperatures have increased by a factor of 10. Progress has been made in the development of new diagnostics of plasma behavior, and these diagnostics have, in turn, led to a deeper understanding of the behavior of fusion plasmas. New methods have been developed to heat fusion plasmas and to drive electrical currents in these plasmas noninductively using intense neutral beams and radio-frequency electromagnetic waves. These methods of current drive could eventually permit the operation of a steady-state fusion reactor. New operating regimes with improved plasma confinement have been discovered, such as the so-called "high-confinement" and "very-high-confinement" modes. There has been progress in the understanding of plasma stability as well as in understanding the interface between the plasma edge and the material walls of the confinement vessel.

A key element in the magnetic confinement fusion program is the development of the International Thermonuclear Experimental Reactor (ITER). This device will be designed to test elements of reactor-relevant plasma science not possible by other means such as the physics of ignition. However, there are many other plasma processes relevant to controlled fusion that will not be able to be addressed effectively by the ITER program. The physics of the edge plasmas in tokamaks needs to be better understood. Advanced modes of tokamak operation at very long pulse lengths will be studied in the Tokamak Physics Experiment (TPX), now planned as a national facility for such studies. Finding improved methods of removing large quantities of heat from the plasma edge is an immediate problem. The efficient production of self-generated plasma currents by high plasma pressures (so-called "bootstrap currents") is an important goal of advanced tokamak configurations that is not likely to be studied efficiently in the ITER program. Experiment and theory should continue in the search for optimized geometries and operating conditions to improve reactor efficiency and power-handling capabilities.

Crucial to the operation of a fusion device is the transport of particles and energy by plasma turbulence, and turbulent transport has been the dominant transport mechanism in all magnetically confined fusion plasmas to date. There is, as yet, only an extremely limited first-principles understanding of the turbulence in fusion plasmas and the resulting transport. Any predictive capability that does exist is based on empirical "scaling laws" that must be validated when applied outside the operating parameter range of present and past fusion devices. A quantitative understanding of this transport and the ability to control it could potentially lead to improved reactor performance and reduced size and cost.

This fundamental base of plasma science is crucial not only for the efficient development of a successful fusion reactor, but also for quantitative understanding of fusion-related plasma science, which will continue to be important in maximizing the competitiveness of fusion power in the decades to follow.

The panel recommends that there be established a coordinated research program in fusion-relevant plasma physics. This will require a range of project sizes, in order to optimize the particular experiments to study the relevant plasma processes. Experimental research is most efficiently done on the smallest scale possible. This allows the greatest flexibility in making changes, as required by new results and discoveries, as well as the greatest exploration of the relevant parameter ranges at minimum cost. Many fundamental questions in basic plasma science should be addressed by small experiments that, in many cases, are specifically designed for a particular purpose. Other questions can be addressed only in larger devices. To study the effects of fusion products (e.g., alpha particles at energies of a few million electron volts) on fusion plasmas, reactor-sized devices, such as the Tokamak Fusion Test Reactor or the Joint European Torus, are required. Thus, a coordinated program of fusion plasma research will require a range of devices and programs, from small, basic experiments that isolate and address fundamental questions in plasma science to experiments on the largest fusion devices.

If the present trend toward large experiments continues without adequate attention paid to a broader base of experimental research facilities, a dangerous gap will develop in our ability to address the wide range of questions important to fusion-relevant plasma physics. Many important questions in fusion plasma physics might be more appropriately addressed by smaller, long-term research programs dedicated to isolating and studying fundamental plasma phenomena in a more complete and systematic manner.

The panel recommends that the program in magnetic confinement fusion include support for a range of projects, with the sizes chosen to best suit the particular plasma problem. Provision needs to be made for research on fusion-relevant basic plasma science. The details of this recommendation are given below in Part II.

Beams, Accelerators, and Coherent Radiation Sources

Until now, progress in this area has been driven by many important defense applications. Given recent changes in world politics, the need for such programs has greatly decreased. Military applications aside, this area of plasma science has a wide variety of important technological applications. Examples of uses of intense electron beams include the bulk sterilization of medical products and food, toxic waste destruction via oxidation, the processing of advanced materials, and new welding techniques. Free-electron laser radiation sources offer the possibility of providing intense, tunable sources of electromagnetic radiation in virtually all parts of the electromagnetic spectrum from the far infrared to x-ray wavelengths. They have a variety of potential applications in medicine and industry. The development of x-ray lasers is in its infancy but holds promise for many important practical applications.

To effectively pursue such applications will require a coordinated research and development effort. The panel recommends that beams, accelerators, and coherent radiation sources be given high priority for "defense conversion" funding.

Space Plasmas

Space plasma physics is concerned with the observation and understanding of naturally occurring solar-system plasmas. It is an evolutionary field, and progress has been achieved incrementally. The space plasma medium extends from the ionosphere of Earth to the far reaches of the solar system and encompasses plasmas of many types. Portions of this domain, such as the magnetosphere of Uranus, have experienced only brief, exploratory coverage, while others, like Earth's ionosphere and magnetosphere, have been investigated relatively thoroughly. In the case of the former, we are still at the stage of trying to deduce gross plasma structure from limited data; with the latter, we are in the process of understanding specific mechanisms that are responsible for the observed morphology. Occasionally, an entirely new physical situation is encountered, unlike anything previously observed in either space or in the laboratory, and this opens new scientific vistas. One example is the dusty plasmas of comets and planetary rings that are dominated by the dynamics of charged macroparticles for which gravitational and electromagnetic effects are of comparable importance.

There are many applications of plasma physics to space science, ranging from the development of plasma thrusters for spacecraft propulsion to "space weather" prediction in the magnetosphere and ionosphere, which has important consequences for physical phenomena on Earth, such as global communications. To some extent, space plasma physics draws upon the vast body of knowledge accrued through the laboratory program for the analysis, interpretation, and modeling of phenomena. Frequently, however, the parameters and the nature of boundary conditions are such as to render the space plasma physics unique, and necessitate entirely new theory or computer modeling. Thus, space plasma physics also contributes to the expansion of our knowledge of basic plasma physics. For example, our extensive understanding of collisionless shocks is based largely on space plasma studies of the Earth's bow shock. In the past, space plasmas have also been used as media in which to study phenomena of both applied and intrinsic interest and importance. However, these aspects of space plasma physics have now been deemphasized programmatically to the point of virtual extinction.

Observation is central to space plasma physics. Although observations are expensive to make, especially those requiring spaceflight, advancement in the field will continue to rely heavily on carefully planned and judiciously selected experiments to provide data that underlie new and refine old ideas. Technological improvements in detection systems and data handling capabilities can be expected to provide increasingly complete and accurate data on which to base models and theories. Recent progress in this area has been impressive, and the prospects for the future are very good.

The ambient space plasma can be modified by a number of techniques, including the injection of waves and particle beams, the injection of plasma and neutral gas, and perturbation by space vehicles. Such perturbations provide opportunities to isolate and study space plasma effects in detail and to create space plasmas relevant to other regions of space. Of particular concern to the panel is the fact that programs in this area of active, space plasma experimentation have recently been deemphasized by the funding agencies, and the panel recommends that this trend be reversed.

Given the spatial and temporal intermittencies of space plasma measurements, a program in laboratory experiments to study space plasma phenomena could be of great benefit. Such experiments have been supported in the past only to an extremely limited degree, due in large part to the fact that the design of experiments with appropriate scaling to space conditions is difficult in laboratory-sized devices. Advances in laboratory plasma experimentation have now progressed to the point that relevant plasma processes can be investigated in the laboratory with a degree of control, precision, and repeatability not achievable in situ. The panel recommends that an initiative be created for the support of laboratory experiments relevant to space plasmas.

Understanding space plasma phenomena frequently requires a combination of extensive data analysis, theory, modeling, and laboratory experiments, in addition to in situ observation. There is concern that in response to the pressure of escalating costs for observations, support for these other aspects of space plasma science has shrunk to unhealthy levels. The panel recommends that NASA and NSF fund a vigorous observational program, including both in situ and ground-based facilities, properly balanced with complementary programs of theory, modeling, and laboratory experiments.

Astrophysical Plasmas

Plasma physics is relevant to almost every area of astrophysics, ranging from stellar and interstellar plasmas to star clusters and galaxies. The potential contribution of plasma physics to astrophysics has been limited by the fact that plasma physics is not yet part of the standard graduate astrophysics curriculum, and this should be changed. Also of concern to the panel is the fact that plasma astrophysics does not have a distinct home in any of the federal funding agencies.

Examples of plasma astrophysics where there has been significant progress include models of magnetized accretion disks and associated jets and winds, including the effects of relativity, strong magnetic fields, rapid rotation, and magnetohydrodynamic waves and instabilities. Mechanisms of particle acceleration in plasma shockwaves have been clarified that are relevant to the acceleration of cosmic rays in the interstellar medium. Models have also been developed to describe the convective fluid motion in stars, including crucial effects arising from the presence of strong magnetic fields on the flow of stellar material.

While there has been progress in plasma astrophysics, many fundamental problems need to be addressed. These include the description of dense stellar plasmas with temperatures in excess of 20 million Kelvin and densities 10 to 100 times solid density. Other important problems involve turbulent plasmas, the origin of the magnetic fields in the universe, and magnetic field line reconnection.

Plasma astrophysics is not yet recognized as a coherent discipline by any federal funding agency. Yet, plasma astrophysics deals with phenomena that are important to virtually all aspects of astronomy and astrophysics, and many of these problems are central to basic plasma physics. The panel recommends that there be established interdisciplinary programs in the National Aeronautics and Space Administration and in the National Science Foundation to fund research in plasma astrophysics, including research on basic plasma processes relevant to astrophysical systems but not tied to any particular application.

CENTRAL MESSAGES OF THIS REPORT

The panel was charged with assessment of the state of plasma science in the United States and evaluation of its potential to contribute to the technology base of our society. It was further charged with assessing the institutional infrastructure in which plasma science is conducted, identifying changes that would improve the research and educational effort, and making recommendations to federal agencies and to the community to address these issues.

The theme of this report is that, although plasmas are pervasive in nature and many of the applications of plasma science are being pursued and exploited effectively, plasma science is not adequately recognized as a discipline. Consequently, there is not an effective structure in place to develop the basic science that underlies its many applications. The potential contributions of plasma science to our society would benefit greatly by a coordinated effort of support for fundamental research, not tied to specific programs but designed to establish this basic scientific foundation.

This report describes many of the developments in theory and experiment that have led to important industrial applications, significant commercial and residential uses, and a deeper understanding of the universe. While delineating the many successes and identifying the exciting and potentially critical challenges, this report is an expression of concern. Applications depend on development, which in turn depends upon a fundamental understanding of the underlying science—the sine qua non of new development. It is the view of experts in the field that this fundamental understanding is, in most cases, best obtained by individual and small-group experimental and theoretical efforts, typical of university-scale programs. The problem is not that there is a gross imbalance in the total funding going into plasma science and technology, but that there has been a gradual, long-term decrease in support for fundamental research in plasma science. The result is that there is now a clear need for such support, particularly in the areas of small-scale, basic plasma experiments and complementary small-group theoretical efforts.

As discussed above, a wide variety of programs pursue the applications of plasma science, including those in space, fusion, and the plasma processing of materials; yet there is great commonality of the underlying science. Even in the context of a particular application, there are often several programs, frequently spread across more than one federal agency. More often than not, this diversity is justified and healthy. However, coordination of research efforts is vital to eliminate duplication, to make the most effective use of resources by maintaining complementary programs, and to ensure that all of the critical problems are being addressed.[4]

A prime example of the existing lack of coordination in plasma research is the fact that *no* agency or agencies have yet assumed the responsibility for basic research in plasma science.

Thus, the central messages of this report are threefold:

1. Small-scale research provides much of the fundamental base for plasma science;
2. Such individual-investigator and small-group research is in need of support; and

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[4] One beneficial effort of this type is the recent, informal coordination of experimental space plasma research by the National Aeronautics and Space Administration, the Office of Naval Research, and the National Science Foundation.

3. There is a need for increased coordination of federally funded plasma science research.

These conclusions coincide with the principal findings and recommendations of the Brinkman report,[5] which was an overall assessment of the future of plasma physics in the United States:

> Direct support for basic laboratory plasma-physics research has practically vanished in the United States. The number of fundamental investigations of plasma behavior in research centers is small, and only a handful of universities receive support for basic research in plasma physics. A striking example is the minimal support for basic research in laboratory plasmas by the National Science Foundation. . . . Because fundamental understanding of plasma properties precedes the discovery of new applications, and because basic plasma research can be expected to lead to exciting new discoveries, increased support for basic research in plasma physics is strongly recommended.

If anything, the state of basic plasma science has worsened in the nine years since the Brinkman report was published. This situation can be remedied only by the creation of a coherent and coordinated plan for the support of basic plasma science.

CONCLUSIONS AND RECOMMENDATIONS

Ongoing research and development programs in the United States have produced important advances in plasma-related science and technology. Plasma science holds promise of further progress in the future, including advanced methods of processing materials, better methods for cleaning up environmental hazards and mitigating the effects of deleterious chemicals, new methods of accelerating particles and producing electromagnetic radiation, progress toward fusion energy, and improved understanding of our space environment and the astrophysical media of the universe. Thus, plasma science can have a significant impact on many disciplines and technologies, including those directly linked to industrial growth. This impact, however, is critically dependent on the support of basic plasma science. It will be important to effect some shift of research funds to this area because of its close relation to applications.

To properly pursue its potential, the United States must create and maintain a coherent and coordinated program of research and technological development in plasma science. Currently, support for basic plasma science is mostly for small programs, found in many agencies, and not coordinated among agencies. The Department of Energy has large programs in the development of fusion energy, by both magnetic and inertial confinement schemes, but it has no unit in the Basic Energy Sciences Division to provide support for the study of the fundamentals of plasma science. The National Science Foundation (NSF) is viewed as the supporter of basic science in universities, and there are several quite small plasma programs scattered throughout the agency. However, basic plasma science has no identified home in NSF and, thus, no specified coordinating and review point and no sponsor.

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[5] See footnote 2, p. 15.

Below, the panel recommends increased support of university-scale experimental research in basic plasma science in the amount of $15 million per year. The justification for this amount is discussed above and in Chapter 8, Basic Plasma Experiments. Although it may seem that this could have only a small influence on a field with an annual budget in excess of $400 million, the expenditure on other than the largest applications, fusion and space plasmas, is less than 10% of this amount.

Consequently, an investment of $15 million on basic experiments can be expected to provide an important stimulus to the entire field. It can also be expected to have a multiplicative effect in that the results in basic plasma research will provide the foundation for research more closely related to all of the applications, including space and fusion.

While many successful programs in plasma science are currently under way, there is a lack of support for the basic aspects of plasma research, particularly where the payoff to a specific program cannot be justified in the near term. The development of plasma science would be improved substantially by its recognition as a scientific discipline.

Given these findings and conclusions, the panel recommends the following six actions:

1. To reinvigorate basic plasma science in the most efficient and cost-effective way, emphasis should be placed on university-scale research programs.
2. To ensure the continued availability of the basic knowledge that is needed for the development of applications, the National Science Foundation should provide increased support for basic plasma science.
3. To aid the development of fusion and other energy-related programs now supported by the Department of Energy, the Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, should provide increased support for basic experimental plasma science. Such emphasis would leverage the DOE's present investment in plasma science and would strengthen investigations in other energy-related areas of plasma science and technology.
4. Approximately $15 million per year for university-scale experiments should be provided, and continued in future years, to effectively redress the current lack of support for fundamental plasma science, which is a central concern of this report. Furthermore, individual-investigator and small-group research, including theory and modeling as well as experiments, needs special help, and small amounts of funding could be life-saving. Funding for these

activities should come from existing programs that depend on plasma science. A reassessment of the relative allocation of funds between larger, focused research programs and individual-investigator and small-group activities should be undertaken.

5. The agencies supporting plasma science should cooperate to coordinate plasma science policy and funding.

6. Members of the plasma community in industry and academe should work aggressively for tenure-track recognition of plasma science as an academic discipline, and work with university faculty and administrators to provide courses in basic plasma science at the senior undergraduate level.

PART II

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Topical Areas

1

Low-Temperature Plasmas

INTRODUCTION

During the last half century, low-temperature plasmas have made a dramatic impact on society, significantly improved the quality of life, and provided challenging scientific problems. Examples are the fluorescent lights that can be found in almost every home in America; high-power switches that control the electrical grid of the United States and divert electrical power on command; gas discharge lasers, including the red He-Ne laser, which was the first gas laser invented, and the high-power, infrared, CO_2 lasers that are used daily in surgery and metal working; and plasma sources that provide positive and negative ions for ion-beam accelerators. These ion sources are used to implant ions into materials, including semiconductor chips for the computer industry, and to harden bearings to increase the life and reliability of high-performance engines. Provided the opportunity, the field of low-temperature plasmas will continue to make significant contributions.

Based on the preceding paragraph, it is not surprising that low-temperature plasmas are important in many disciplines. Typically, they are high-pressure collision-dominated plasmas that have average electron energies of 1-10 eV. The purity of the gas is often important, and the physics and chemistry of the excited atomic states dominate the discharge characteristics. In industrial applications, the stability of the discharge frequently impacts the design and utility of the process, and the heterogeneous wall chemistry often impacts its reproducibility and reliability.

Unfortunately, because basic research in this area has been neglected for many years, there is a severe lack of quantitative and experimental understanding of a wide range of phenomena that occur in low-temperature collision-dominated plasmas. Most low-temperature plasma applications involve complex reactions between electrons and a host of atomic, molecular, and ionic species. These species are found in highly excited states not encountered in nonplasma environments. Operation of plasmas in applications ranging from lasers to materials processing and lighting requires optimization of the densities of these species. Scientists modeling these systems require a broader range of diagnostics to characterize species densities in benchmark plasmas, and more powerful methods for measuring, calculating, or approximating the cross sections that dominate the rate equations.

In some cases, such as microwave breakdown, the positive column of dc metal-vapor rare-gas discharges, and wall-stabilized arcs, researchers have obtained experimental data, theoretical understanding, and predictive models. However, much of this basic research was performed before 1960. In some cases with immediate industrial and government applications the information was updated in the 1970s, using modern experimental and modeling techniques. Examples include fluorescent lamps, high-intensity lamps, electron-beam-controlled discharge lasers, some specific plasma processes, and arcs (e.g., in discharge-limiting situations, such as transport in weakly ionized swarms and near thermal equilibrium). This research produced a significant improvement in the performance of devices using these plasmas. Recent research was driven by interest in high-power lasers for ballistic missile defense. The decline of interest in that use has severely reduced related funding.

In other areas, there has been limited progress during the last 30 years, including understanding phenomena such as collisional discharges in magnetic fields in the presence of boundaries, transient discharges and sheaths, discharge stability, and plasma interactions with practical surfaces. For example, recently there has been much interest in the dc cathode fall, since modeling and experiments are much further ahead for bulk-phase plasmas than for cases, such as the cathode fall, in which plasma contact with surfaces is important.

Lack of research support in the physics of low-temperature plasmas has resulted in a low level of training in collision-dominated low-temperature plasmas and in the training of engineers and physicists for plasma processing. No federal agency claims responsibility for this area.

The existing support has emphasized short-term goals and work only on current government- and industry-related topics. In FY 1991, there were only two long-term projects, and neither is currently funded. It is our understanding that since the beginning of FY 1992, there has been essentially no low-temperature plasma research project with more than a one-year time scale, since research in this area is dominated by the current needs of the radio-frequency plasma processing and lighting industries. This short time scale severely discourages new, innovative, or thorough research. Novel experimental and modeling techniques should be developed to explore new areas and provide more quantitative work in existing areas.

A serious problem in low-temperature collision-dominated plasmas has been the lack of reproducible experimental verification of theoretical predictions. This is partly due to the critical dependence of the relevant phenomena on surface conditions and gas purity. It is also due to the fact that the models are too limited and qualitative to be tested or to be of general use. While gas purification techniques have been known for many years, the role of impurity effects in practical systems is often poorly understood. The problem of the reproducibility of practical surfaces is very difficult, and few useful and successful recipes exist. Even fewer techniques exist for characterizing practical surfaces with regard to their interactions with plasmas. As a result, most of the successful quantitative gas-discharge investigations are of phenomena that are relatively free of surface effects (i.e., microwave breakdown, the positive column, swarm transport, and near-equilibrium radiation). However, given adequate funds, more realistic models could be developed to investigate these complex phenomena with modern computer facilities.

The Japanese government has long supported an active program in basic gas discharge research, particularly in its engineering schools. Japanese research is recognized internationally for its quality and impact. In spite of a major focus on plasma processing, many Japanese faculty still devote a significant fraction of their time and resources to basic, undirected research. In recent years the French government also has supported a large effort in low-temperature collision-dominated plasma research, which has produced a large fraction of the invited papers at recent international meetings.

To change the situation in the United States will require strong support for research in applied physics and engineering in the area of the basic physics of low-temperature plasmas. Experimental programs emphasizing quantitative and reproducible results will be necessary to properly test the predictions of theoretical models. Improved understanding of these plasmas is necessary for applications such as plasma processing and environmental cleanup. This basic research can also be expected to yield innovative experimental techniques and novel modeling methods, and it will provide highly trained scientists and engineers in low-temperature plasma science.

That low-temperature plasmas are crucial in so many technologies is both a strength and a weakness. These plasmas are indispensable in today's highly technical world, but since they are useful in many apparently disconnected disciplines, no agency has taken responsibility for research in low-temperature plasmas.

This chapter focuses on the following important areas of low-temperature plasma physics: lighting, gas discharge lasers, plasma isotope separation, space propulsion, magnetohydrodynamics, and the use of plasmas for pollution control and reduction. Another major area is plasma processing, which was addressed in detail in the recent report of the National Research Council (NRC) Panel on Plasma Processing of Materials.[1] The findings and recommendations of that study are summarized below in the section "Plasma Processing of Materials."

LIGHTING

Lighting has been one of the principal areas contributing to the understanding of low-temperature plasmas, and historically it has been responsible for much of the low-temperature plasma research in industry. Unfortunately, due to the severe recession of the late 1980s and early 1990s, this research effort has declined rapidly. Westinghouse and GTE-Sylvania have sold their lighting divisions to foreign investors, leaving General Electric the only large U.S. lighting company. By contrast, low-temperature plasma research in the Far East has been increasing rapidly; it is now three to four times larger than that in the United States.

Important contributions from lighting in the past 10 years include the control and modification of the electron-energy distribution function and novel laser diagnostics that provide valuable microscopic information about discharge parameters. Other important contributions include sophisticated and predictive models of lighting discharges and an understanding of the effects of isotopic gas mixtures in low-pressure mercury rare-gas discharges.

Major technological innovations have been made in the last decade in many areas. They include lower-power compact fluorescent and high-intensity discharges (HID), a variety of electrodeless discharges such as microwave, rf, and surface wave discharges for practical lighting applications, and the electronic ballasting of light sources. Other important innovations include an improved understanding of heterogeneous chemistry, resulting in superior performance and better compatibility with existing and novel materials, and the development of a systems approach to light sources that integrates principles of plasma discharges, materials, electronics, and homogeneous and heterogeneous chemistry.

Although most of the R&D for lighting plasmas is performed by the lighting industry, the field has also benefited from advances in other disciplines. For example, solid-state, plasma processing, and materials advances made in other industries and in academic and research institutions have contributed to the progress in the lighting industry.

A fundamental understanding of many processes is necessary for the lighting industry to design and fabricate higher-efficiency lamps. Therefore, university and government research has and will continue to impact the industry. Areas of research include, for example, local thermodynamic equilibrium (LTE) and non-LTE low-temperature plasmas, sophisticated diagnostics, and measurement of atomic and molecular cross-section data for electron-impact processes of importance to the lighting industry.

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[1] National Research Council, *Plasma Processing of Materials: Scientific Opportunities and Technological Challenges*, National Academy Press, Washington, D.C., 1991.

Because industrial R&D goals have become increasingly short-term over the last 10 years, it is important that the academic and government communities initiate longer-term R&D in plasmas related to lighting. Examples of research and development that would be of great benefit to the lighting industry include the application of massive computation techniques to lighting problems and improved diagnostics that will provide detailed information about the behavior of lighting plasmas. New approaches to modeling and probing complex sheaths associated with thermionic electrodes, for example, would help the lighting industry reduce mercury and thorium usage and would also increase lamp efficiency and life. Other important topics include methods to obtain higher conversion efficiency of electrical energy into radiation and the evaluation and exploitation of solid-state sources for lighting applications.

Scientific opportunities in lighting plasmas include the exploration of novel ways of producing monoenergetic or narrow electron-energy distributions in discharges to selectively excite electronic states, resulting in the more efficient production of radiation and the reduction of long-wavelength emission and thereby enhancing visible emission, using the principles of quantum electrodynamics and quantum interference.

Radiation from lamps can have important applications in environmental cleanup and other areas, including water purification with light, promoting algae growth with special metal-halide sources to reduce heavy metal concentrations in water, accelerating food growth, and a variety of display applications.

Lighting plasmas are synergistic with the fields of plasma deposition and etching, materials science, electronics, and lasers. However, increased scientific productivity in this area will require new basic experimental facilities.

GAS DISCHARGE LASERS

The field of gas discharge lasers has had considerable government support over the last 40 years. Strong support in the 1970s and 1980s led to an improved understanding of the basic phenomena in high-pressure plasmas, including electron-impact excitation cross sections of vibrational and electronic excited states, the physics of the stability of high-pressure discharges, and the homogeneous chemistry and products of excited-state reactions. Advances in the understanding of discharge physics include improved understanding and predictive capability of the discharge parameters and the stability of the plasma, discovery of the dominant impact that excited states have on discharge physics and laser chemistry, and an increased knowledge of electronic kinetics and the interaction between secondary electrons and excited states.

This research made significant contributions to advancing the state of the technology by increasing the efficiencies of lasers from a fraction of a percent to 10%. This improvement was particularly dramatic in excimer lasers. Examples include the rare-gas lasers that radiate in the vacuum ultraviolet (VUV), the rare-gas halide lasers that lase in the ultraviolet (UV), the rare-gas triatomic excimers that have broadband emission in the visible, and the metal excimers that emit in the visible and UV.

Gas lasers have produced many important technological capabilities. Examples include optical lithography, where rare-gas fluoride lasers have extended the resolution to less than 0.5 μm; laser working of metals, where high power CO_2 lasers are now used routinely for welding, cutting, and marking in industry; and medicine, where lower-power gas lasers have made a significant impact, including the use of surgical CO_2 lasers and excimer lasers for treating eyes and occluded arteries.

The combined gas laser market is presently of the order of $300 million and is predicted to grow at an annual rate of approximately 5%. There are also other emerging uses for these lasers, such as LIDAR (laser radar) for airports that can measure the location of wind shear and thereby increase the safety of air travel, and laser-produced x-ray sources for microlithography.

PLASMA ISOTOPE SEPARATION

Funding for plasma isotope separation has decreased dramatically in the post-Cold War era. Isotope separation has been actively investigated for the last 20 years, principally by plasma centrifuge, laser (AVLIS), and ion cyclotron resonance techniques. Of these methods, the AVLIS program at Lawrence Livermore National Laboratory has been the most strongly supported. The results are classified. Briefly, the separation process involves the selective ionization of one isotope and the subsequent collection of this ion. Lasers are used to ionize the desired isotopes, which form a low-temperature plasma. Plasma physics issues that have to be addressed include excited and ionic species reactions, homogeneous chemistry, and the physics and chemistry of the sheath near the collection electrodes. More conventional plasma isotope separation has been investigated on a much smaller scale by several groups including the FOM Institute for Plasma Physics in the Netherlands, Yale University, the Max Planck Institute in Germany, the Sydney University School of Plasma Physics in Australia, the National Space Research Institute in Brazil, and TRW in the United States.

There are important uses of isotope separation besides nuclear fuels enrichment, including medical diagnostics, chemistry, and basic research. Thus, the development of plasma centrifuge technology offers a number of potential opportunities. The demand for stable, enriched isotopes for medical applications grows each year. The plasma centrifuge offers an improved means of meeting this need. However, at present, many important basic plasma phenomena remain to be understood in the rotating plasmas utilized in plasma centrifuges, and further research in this area will be necessary to fully exploit their potential. In addition to isotope separation, the plasmas developed for plasma centrifuges can also be expected to be useful for other applications, including use in imploding, "z-pinch," plasma x-ray sources and in plasma switches.

PLASMAS FOR ELECTRIC PROPULSION OF SPACE VEHICLES

Space electric propulsion has been studied for the last three decades. Concepts that have been investigated include expanding electrically heated plasmas, accelerating plasmas with thrusters and plasma guns, accelerating ions, and laser propulsion. Research groups have included TRW in Redondo Beach, California; Avco Everett Research Laboratory in Everett, Massachusetts; the National Aeronautics and Space Administration (NASA) Lewis Research Center; and several university research groups. Electric plasma propulsion requires less fuel mass than chemical systems, potentially making launching of satellites and space exploration less expensive. Decreasing the weight of fuel and hence the overall payload could have a significant impact on the $9.5 billion currently spent annually for launches: $5 billion by the Department of Defense (DOD), $3 billion by NASA, and $1.5 billion by industry.

A plasma propulsion device (an ion accelerator) has been tested and has worked successfully in space for 13 years. A key issue for any such device that is launched into space is its reliability and longevity in both its on-the-shelf and operating lives. This makes the use of electrodes problematic. TRW has been pursuing electrodeless thrusters, and these plasma accelerators potentially could satisfy the demanding reliability requirements of space qualifiable systems. Other areas that require research include a better understanding of the plasma, identification of the appropriate gas or fuel, and matching the electrical driver to the nonlinear plasma load. There is presently a pressing need for low-power (e.g., of order 100 W) thrusters for long-term maintenance of orbits. The requirements for interplanetary missions will require significantly higher-power thrusters. The development of space plasma propulsion systems also is synergistic with other applications. For example, plasma accelerators can be used in plasma processing and in the simulation of space plasmas to determine the chemistry of these reactive media on satellites.

MAGNETOHYDRODYNAMICS

The branch of magnetohydrodynamics (MHD) of interest here is that concerned with plasmas at low temperatures (2000-10,000 K) and high pressures (1-10 atm). Most of the current work in this area is engineering development rather than scientific research. These efforts have focused primarily on electrical power generation, with some effort directed toward space vehicle thrusters. The power generation community is mostly industrial, and much of this work has been performed by Textron Defense Systems (formerly the Avco Everett Research Laboratory). There is some university involvement, such as the work at the University of Tennessee Space Institute. Workers in the field are generally engineers with plasma and fluid, thermoscience, mechanical engineering, or electrical engineering backgrounds.

There are several technological opportunities for applications, including electric power plants; multimegawatt portable power supplies for land, air, or space uses; high-enthalpy test facilities for testing high-speed propulsion systems; magnetoplasmadynamic (MPD) thrusters for space vehicles; and MHD boost of oxygen-fuel jets in coating applications. The ultimate users of MHD power generation technology may be the utilities and independent power producers who generate electric power and who seek economic and environmentally benign methods of generating it.

Major achievements during the past 10 years include the development of equipment capable of operating for long durations and a greater understanding of the physical phenomena associated with the corrosion and erosion of plasma-facing surfaces. Industry has played a key role in the engineering development of components and subsystems of power-generating systems for proof-of-concept demonstrations. The ultimate objective of this work is commercialization of the technology.

The science community involved in this area is small. There is a need for a better understanding of basic MHD phenomena. Specific needs include a detailed understanding of conditions that lead to plasma instabilities at high power densities; a better understanding and analysis of electric discharges in flowing, reacting gases; analysis and experiments to better understand electrode and boundary layer phenomena in the presence of strong magnetic fields, slag-layer shorting effects, and associated electrical nonuniformities; and a better understanding of scaling laws. Additional research could also foster the development of high-temperature, nonslagging channel walls and high-temperature direct-fired air preheaters.

PLASMAS FOR POLLUTION CONTROL AND REDUCTION

With the increased concern for the environment, the use of plasmas for pollution control and reduction is predicted to be an area of considerable growth in the next decade. In particular, American industry is beginning to realize the importance of low-temperature plasmas for pollution control (e.g., flue gas treatment, air toxics treatment), while international efforts at pilot-plant scale are much more advanced. At present, researchers in this community are focusing mainly on studies of the plasma chemistry and discharge physics of nonequilibrium plasmas. The plasmas are usually created by electrical or electron-beam-driven discharges. This field is very old in terms of the phenomenological understanding of the phenomena involved and the identification of potential practical applications. Modern research in this field is highly applications oriented, with basic research focused primarily on the measurement and computer-based modeling of transient events. Applied research in this area is divided between a fundamental approach, involving basic discharge physics and plasma chemistry, and an Edisonian approach, centered on plasma-based pollution control devices.

Nonequilibrium plasma technology has been applied to the chemical processing of gaseous media for more than a century. Two major applications are chemical synthesis, exemplified by ozone generation, and the removal of undesirable compounds from flue gases, exemplified by the electrostatic precipitator. During the past two decades, interest in applying nonequilibrium plasmas to the removal of hazardous chemicals from gaseous media has been growing, particularly because of heightened concerns over the pollution of our environment and a growing body of environmental regulations. These more recent applications have involved efforts to destroy toxic chemical agents, to remove harmful acid rain gases such as sulfurous and nitrous oxides, and to treat other environmentally hazardous hydrocarbon and halocarbon compounds. Major contributions in the last 10 years include the decontamination of wastewater, flue/stack gas processing for SO_2 and NO_x reduction, military applications (nerve agent destruction), and toxic chemical/vapor processing.

Industry is a potential user and market for plasma technology for pollution control. The utility industry is faced with more stringent environmental regulations, which demand improved technology for effluent cleanup (both for power plants and for utility customers). Industry can play a strong role as an advocate for technology development and as a technical contributor by working cooperatively with researchers in the field on applications. Many institutions are actively involved in this area, including Lawrence Livermore National Laboratory, Los Alamos National Laboratory, Sandia National Laboratories, and several universities and industrial laboratories. To date, funding in this area has been small, but given the increasing concern about environmental issues, it could increase dramatically in the next decade.

There is much that has to be learned before low-temperature plasmas can be used in the cleanup and preservation of the environment. This includes developing a better database for plasma chemical processes, reaction-rate constants, and the resulting products, and developing diagnostics to determine the physics and chemistry of the cleanup process. There are many scientific and technical opportunities, including developing basic plasma data on the reaction of excited states and radicals with various contaminants and sophisticated modeling of the physics and chemistry of plasmas as they apply to the cleanup problem. Fortunately, there are many facilities in the various national laboratories, universities, and industry that can be used to perform the initial proof-of-principle experiments. There is a strong synergism between environmental applications and the many other disciplines that also depend on low-temperature plasmas.

PLASMA PROCESSING OF MATERIALS

Plasmas used for the processing of materials affect several of the largest manufacturing industries, including national defense, automobiles, biomedicine, computers, waste management, paper, textiles, aerospace, and telecommunications. The importance of plasma processing to the electronics industry is illustrated in Figure 1.1. An NRC study reviewed plasma processing of materials in detail in 1991.[2] This section briefly summarizes its findings and recommendations.

Applications of plasma-based systems used to process materials are diverse because of the broad range of plasma conditions, geometries, and excitation methods that may be used. This technology is multidisciplinary and, ideally, the researchers should have a basic knowledge of several scientific disciplines, including elements of electrodynamics, atomic science, surface science, computer science, and industrial process control. The impact of—and urgent need for—plasma-based materials processing is overwhelming for the electronics industry.

In its report, the NRC study panel made the following statements:[3]

- "In recent years, the number of applications requiring plasmas in the processing of materials has increased dramatically. Plasma processing [such as that illustrated in Plate 1] is now indispensable to the fabrication of electronic components and is widely used in the aerospace and other industries. However, the United States is seeing a serious decline in plasma reactor development that is critical to plasma processing steps in the manufacture of VLSI [very large scale integrated] microelectronic circuits. In the interest of the U.S. economy and national defense, renewed support for low-energy plasma science is imperative." (p. 2)
- "The demand for technology development is outstripping scientific understanding of many low-energy plasma processes. The central scientific problem underlying plasma processing concerns the interaction of low-energy collisional plasmas with solid surfaces. Understanding this problem requires knowledge and expertise drawn from plasma physics, atomic physics, condensed matter physics, chemistry, chemical engineering, electrical engineering, materials science, computer science, and computer engineering. In the absence of a coordinated approach, the diversity of the applications and of the science tends to diffuse the focus of both." (p. 2)
- "Currently, computer-based modeling and plasma simulation are inadequate for developing plasma reactors. As a result, the detailed descriptions required to guide the transfer of processes from one reactor to another or to scale

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[2] See footnote 1, p. 36.

[3] See footnote 1, p. 36.

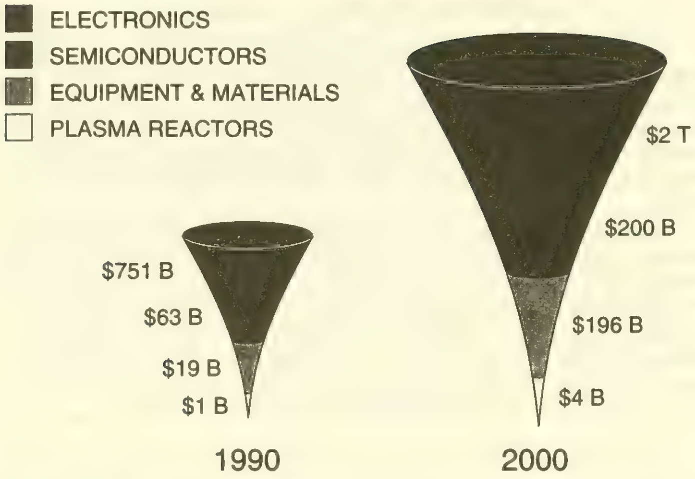

FIGURE 1.1 The world electronics "food chain." Although revenues for plasma technology are a small portion of the world electronics market, plasma technology is a critical component upon which the industry rests. Note that the plasma reactor business is expected to quadruple in this decade. (Courtesy of R.A. Gottscho. Adapted from the National Advisory Committee on Semiconductors report, *Preserving the Vital Base*, Arlington, Va., July 1990, and from data in "Semiconductor Equipment Manufacturing and Materials Worldwide," Dataquest, Inc., 1994.)

processes from a small to a large reactor are not available. Until we understand how geometry, electromagnetic design, and plasma-surface interactions affect material properties, the choice of plasma reactor for a given process will not be obvious, and costly trial-and-error methods will continue to be used. Yet there is no fundamental obstacle to improved modeling and simulation nor to the eventual creation of computer-aided design (CAD) tools for designing plasma reactors. The key missing ingredients are the following: (1) A reliable and extensive plasma data base against which the accuracy of simulations of plasmas can be compared. . . . (2) A reliable and extensive input data base for calculating plasma generation, transport, and surface interactions. . . . (3) Efficient numerical algorithms and supercomputers for simulating magnetized plasmas in three dimensions." (p. 3)

- "In the coming decade, custom-designed and custom-manufactured chips, i.e., application-specific integrated circuits (ASICs), will gain an increasing fraction of the world market in microelectronic components. This market, in turn, will belong to the flexible manufacturer who uses a common set of processes and equipment to fabricate many different circuit designs. Such flexibility in processing will result only from real understanding of processes and reactors. On the other hand, plasma processes in use today have been developed using a combination of intuition, empiricism, and statistical optimization. Although it is unlikely that detailed, quantitative, first-principles-based simulation tools will be available for process design in the near future, design aids such as expert systems, which can be used to guide engineers in selecting initial conditions from which the final process is derived, could be developed if gaps in our fundamental understanding of plasma chemistry were filled." (p. 4)

- "Three areas were recognized by the PLSC [Plasma Science Committee] panel as needing concerted, coordinated experimental and theoretical research: surface processes, plasma generation and transport, and plasma-surface interactions. For surface processes, studies using well-controlled reactive beams impinging on well-characterized surfaces are essential for enhancing our understanding and developing mechanistic models. For plasma generation and transport, chemical kinetic data and diagnostic data are needed to augment the basic plasma reactor CAD tool. For studying plasma-surface interactions, there is an urgent need for in situ analytical tools that provide information on surface composition, electronic structure, and material properties." (p. 4)
- "Breakthroughs in understanding the science will be paced by development of tools for the characterization of the systems. To meet the coming demands for flexible device manufacturing, plasma processes will have to be actively and precisely controlled. But today no diagnostic techniques exist that can be used unambiguously to determine material properties related to device yield. Moreover, the parametric models needed to relate diagnostic data to process variable are also lacking." (p. 4)
- "The most serious need in undergraduate education is adequate, modern teaching laboratories. Due to the largely empirical nature of many aspects of plasma processing, proper training in the traditional scientific method, as provided in laboratory classes, is a necessary component of undergraduate education. The Instrumentation and Laboratory Improvement Program sponsored by the National Science Foundation has been partly successful in fulfilling these needs, but it is not sufficient." (p. 5)
- "Research experiences for undergraduates made available through industrial cooperative programs or internships are essential for high-quality technical education. But teachers and professors themselves must first be educated in low-energy plasma science and plasma processing before they can be expected to educate students. Industrial-university links can also help to impart a much needed, longer-term view to industrial research efforts." (p. 5)

Plasma processing of materials is a technology that is of vital importance to several of the largest manufacturing industries in the world. Foremost among these industries is the electronics industry, in which plasma-based processes are indispensable for the manufacture of VLSI microelectronic circuits (or chips). Plasma processing of materials is also a critical technology in the aerospace, automotive, steel, biomedical, and toxic waste management industries. Because plasma processing is an integral part of the infrastructure of so many American industries, it is important for both the economy and the national security that the United States maintain a strong leadership role in this technology.

As in the case of other disciplines that use low-temperature plasmas, there is no centralized agency that takes responsibility for R&D for this area. The NRC plasma processing study determined that there are approximately 14 agencies within the federal government that invest approximately $17 million in plasma process science and technology. It concluded that this funding was inadequate and uncoordinated, given the impact of this vital area on the country.

CONCLUSIONS AND RECOMMENDATIONS

Conclusions

Low-temperature plasma science has significantly improved the quality of our lives. These contributions will continue by providing solutions to several present and future problems and by preserving our industrial base and providing challenging opportunities in the post-Cold War era. Examples of technical areas that will benefit from low-temperature plasmas include the plasma processing of materials, environmental cleanup, "cold" sterilization of medical products, "cold" pasteurization of food, advanced imaging devices that can be used in medicine and in the detection of explosives and drugs, and isotope separation.

Research in low-temperature plasmas has decreased substantially, primarily because the largest source of funding, the federal government, has had a shrinking budget for such activities in the last several years. Research has also been adversely affected by the recent recession and a general move of large U.S. companies to divest themselves of manufacturing.

The shrinking budgets of the last few years have resulted in a sharp decrease in the population of scientists working in low-temperature plasma science. The supply of PhD-level scientists would be sufficient to reverse this trend, if funding were available. If this trend is not reversed, the United States will be creating a future problem.

Recommendations

To fully exploit the potential of low-temperature plasma science and maximize its impact on the many relevant technological applications, the panel recommends that one agency within the government be given the responsibility for coordinating research in low-temperature plasma science. Given the multidisciplinary nature of the field, the Advanced Research Projects Agency (ARPA) and the National Institute of Standards and Technology (NIST) are possible candidate agencies for this responsibility.

The panel recommends that the responsible agency focus on funding low-temperature plasma science. Such projects would include the physics and chemistry of the plasma sheath; plasma stability; electrodeless plasma production; magnetic field effects on plasmas; improved diagnostics to help understand surface and sheath effects and plasma stability; energy and charge transfer from plasmas to particulates; and improved utilization of computers.

2

Nonneutral Plasmas

INTRODUCTION AND BACKGROUND

A nonneutral plasma is a many-body collection of charged particles in which there is not overall charge neutrality. Such systems are characterized by self-electric fields and, in high-current configurations, self-generated magnetic fields. *Single-component plasmas* are an important class of nonneutral plasmas, the most common examples of which are pure electron and pure ion plasmas. For single-component plasmas in cylindrical geometry, there exists a stringent confinement theorem. The practical consequence of this theorem is that, in contrast to electrically neutral plasmas, a magnetized single-component plasma can be confined easily for very long times (e.g., hours). Therefore, thermal equilibrium and controlled departures from equilibrium can be achieved readily. Nonneutral plasmas exhibit a broad range of collective plasma behavior, such as plasma waves, instabilities, and Debye shielding. Moreover, the rotation and self-generated fields in these plasmas can have a significant effect on plasma properties and stability behavior.

In addition to their importance in understanding fundamental aspects of the behavior of many-body charged-particle systems, there are many practical applications of nonneutral plasmas. Examples discussed elsewhere in this report include the generation of coherent radiation by intense charged-particle beams, the development of advanced accelerator concepts, and the stability of electron and ion flow in high-voltage diodes. Other applications include particle-beam fusion, and the stability and propagation of intense charged-particle beams through background plasma or through the atmosphere. This section focuses specifically on single-component plasmas, with emphasis on pure electron and pure ion plasmas. Research and development in this area is likely to have a significant impact on a wide range of important applications, such as new generations of precision clocks; chemical analysis by improved methods of mass spectrometry; and the accumulation, storage, and transportation of antimatter.

Early research on nonneutral plasmas predated by many decades common usage of the term "plasma." For example, efforts to investigate the equilibrium and stability properties of nonneutral electron flow began with Child (1911), and continued with the work of Langmuir (1923), Llewellyn (1941) and Brillouin (1945), and work on beam-type microwave devices in the 1940s and 1950s. During the past 20 years, interest in the physics of single-component plasmas has grown substantially in such diverse areas as the equilibrium, stability, and transport properties of these plasmas; phase transitions in two- and three-dimensional plasmas; astrophysical studies of large-scale nonneutral plasma regions in the magnetospheres of neutron stars; and the development of positron and antiproton ion sources.

In the case of trapped-ion plasmas, an important synergism has developed between atomic physicists and plasma physicists. Atomic physicists have developed methods to confine and study small collections of ions with great precision. With the addition of more particles, issues of collective oscillations and confinement properties of spatially extended, three-dimensional plasmas become relevant and raise a number of important questions. Study of these questions has illuminated fundamental issues in plasma physics. It has resulted in enhanced capabilities in the creation and control of pure ion plasmas for precision measurements of fundamental constants and for applications such as atomic clocks.

A significant fraction of nonneutral plasma research is closely tied to important technological applications. In contrast to the general development of fundamental plasma experiments that has been hindered significantly in the past two decades due to lack of support, experimental progress in nonneutral plasma research has been excellent, which has stimulated much progress in the theory of nonneutral plasmas. It is the conclusion of the panel that the relative success of research on nonneutral plasmas was due to a strong and dedicated program of support in this area by the Office of Naval Research, with complementary support from the National Science Foundation and the Department of Energy. The panel concludes that this mode of support for nonneutral plasma research should be considered a model for the support of fundamental plasma experiments in the broader area of neutral plasma research.

RECENT ADVANCES IN NONNEUTRAL PLASMAS

The following summarizes significant advances in the physics of nonneutral plasmas during the past decade.

Electron Plasmas

Much progress has been made in understanding the basic physics of single-component plasmas, including critical aspects of the stability, confinement, and equilibrium of these plasmas. It was shown theoretically that the conservation of canonical angular momentum implies, in the absence of external torques, that a single-component plasma can be confined indefinitely. Soon afterward, it was demonstrated that the confinement of pure electron plasmas for several minutes to hours is relatively easily achievable in laboratory experiments. The confinement times are sufficiently long that the plasma approaches a state of thermal equilibrium.

The existence of these thermal equilibrium states in confined, single-component plasmas distinguishes them from neutral plasmas. A magnetically confined neutral plasma does not remain in a state of spatially isolated, local thermal equilibrium, because collisions between the electrons and ions lead to a diffusive expansion of the plasma across magnetic field lines. In addition, in a neutral plasma there is typically free energy (associated with the relative cross-field flow of electrons and ions) available to drive collective instabilities that produce enhanced transport across the field lines. Such instabilities pose a challenge to the achievement of high-quality confinement in electrically neutral plasmas of interest in fusion. In contrast, a confined, single-component plasma that has come to thermal equilibrium is in a state of minimum free energy and hence is stable. It is also a great advantage theoretically to be able to use thermal equilibrium statistical mechanics to describe the equilibrium state.

Theory predicted that, in a strong magnetic field and at low temperature, the relaxation of the particle velocities to a thermal equilibrium distribution would be constrained by an adiabatic invariant, and as a consequence, the relaxation rate would be exponentially small. Subsequent experiments confirmed this prediction, and now there is good agreement between theory and experiment over eight orders of magnitude in effective magnetic field strength and five orders of magnitude in the scaled relaxation rate.

The well-controlled nature of these plasmas has also permitted precise studies of nonequilibrium states unachievable in other plasmas. For a sufficiently low-density nonneutral plasma, in the limit that transport along magnetic field lines is rapid compared to transport perpendicular to the field, the plasma is described by similar equations (in an isomorphic sense) to those describing an inviscid classical fluid in two dimensions. Charge-density perturbations in a single-component plasma are analogous to vortices in a fluid, and vortex dynamics is an important subject of long-standing interest in fluid dynamics. Recently, this analogy has begun to be exploited to test models of coherent structures and vortex merger with a precision not possible in classical fluids. For example, since the effective viscosity of a pure electron plasma is less by orders of magnitude than the viscosity of a classical fluid, the trajectories of a pair of vortices can be followed for 10^4 or 10^5 orbits before merger occurs. In the case of classical fluids such as water, merger or dissipation typically occurs in a few orbits.

Finally, a nonneutral plasma exhibits a wide range of collective waves and instabilities analogous to those observed in an electrically neutral plasma, appropriately modified by self-field effects. These collective waves and instabilities have been documented extensively in theoretical analyses, and algorithms have been developed for calculating the detailed stability behavior of nonneutral plasma over a wide range of system parameters. In addition, a kinetic stability theorem has been developed that determines a sufficient condition for the nonlinear stability of cylindrically symmetric equilibria to arbitrary-amplitude perturbations, including the influence of strong self-field effects.

Ion Plasmas

Another type of single-component plasma that has been studied extensively is the magnetized, pure ion plasma, confined in a Penning trap, and cooled by laser radiation. In this case, the laser light is used both to cool the ions and to exert a torque on the plasma. This torque has the effect of spinning up the plasma and compressing it. An analytical theory of the collective modes of oscillation in these plasmas has been formulated on the basis of cold fluid theory. This is the first analytical description of the modes of a magnetized, three-dimensional plasma of finite extent with realistic boundary conditions. There is good agreement between theory and the experimental observation of these modes. One result of this increased understanding is that these modes can now be used for the manipulation and confinement of pure ion plasmas. An exact nonlinear theory has been developed for the case of large-amplitude quadrupole modes of oscillation of these plasmas.

In ion plasmas that are laser-cooled to cryogenic temperatures, the average kinetic energy per particle can be made small compared to the average interaction potential energy. (These plasmas are often referred to as strongly coupled plasmas.) The resulting ion clouds can form the analogues of dense liquid and solid phases. (See Figure 2.1.) Theoretical and experimental progress has been made recently in understanding the ordering and equilibrium states of these systems. As the temperature is lowered, theory predicts that the ions arrange themselves in concentric spheroidal shells. These shells are the analogues of crystal planes, except that the planes are deformed into spheroids because of the small plasma size. This shell structure has been observed experimentally, with optical imaging techniques, for plasmas up to about 15 shells. Theory predicts that the sample must contain about 60 shells to result in the structure predicted for plasmas of infinite extent (a body-centered-cubic lattice), but this has not yet been tested experimentally.

Small numbers of ions have also been confined and cooled in Paul traps, which utilize the ponderomotive force from high-frequency electric fields to confine the ions. At low temperature, Coulomb repulsion between the ions causes the ions to crystallize into simple geometrical configurations (Coulomb clusters) whose shapes can be predicted theoretically. These clusters and ordered one-dimensional chains of ions have now been observed and studied in Paul traps. (See figure 2.1a.) As the number of ions is increased, experiments have observed polymorphic phase transitions to more complex lattice structures: first a zigzag arrangement, then a helical chain, and finally a cylindrical shell structure, similar to the spheroidal shells observed in Penning traps. These phase transitions have also been studied theoretically.

Such linear lattice structures also are predicted to occur in chains of ions confined and cooled in a heavy-ion storage ring. In the rest frame of the ions circulating in such a storage ring, the confining forces are nearly the same as those in the linear Paul trap described above.

The theoretical density limit for the confinement of a magnetized, single-component plasma occurs when the square of the plasma frequency is one-half the square of the cyclotron frequency (Brillouin, in 1945). This "Brillouin density limit" has been achieved in pure ion plasmas by using laser radiation to exert torques on the plasma and thereby to compress it. In a "cold" one-component plasma column, the radially outward space-charge and centrifugal forces on a fluid element balance the inward magnetic confining force (i.e., the Lorentz force). This places a limit on the maximum plasma density that can be confined for a given value of magnetic field (i.e., the Brillouin density limit). The rotation frequency at the Brillouin limit is such that the Lorentz force on the plasma particles is just canceled by the Coriolis force, and the plasma is effectively unmagnetized when viewed in the rotating frame. Therefore, at the Brillouin limit, it is possible to study in detail a fundamentally new plasma regime in which the confined plasma is effectively "unmagnetized."

Ion Plasmas in Electron-Beam Ion Traps

The electron-beam ion trap configuration, which was invented in the last decade, uses a magnetically compressed electron beam, with energies in the range of several hundred keV, to ionize, trap, and excite highly charged ions of a wide variety of elements for atomic physics measurements. Electron-beam ion trap devices are capable of producing high-resolution x-ray spectra of nearly stationary ions that have been excited by monoenergetic electrons. One can also vary the energy of the electron beam on a time scale fast compared to that for changes in the ionization states of the ions. Thus, the ions can be excited with electrons of one energy and probed with electrons of a different energy.

These devices have been able to produce one-electron (i.e., hydrogen-like) ions up to nuclear charge $Z = 92$. In the last few years many important measurements have been made utilizing electron beam ion trap devices for atomic phys-

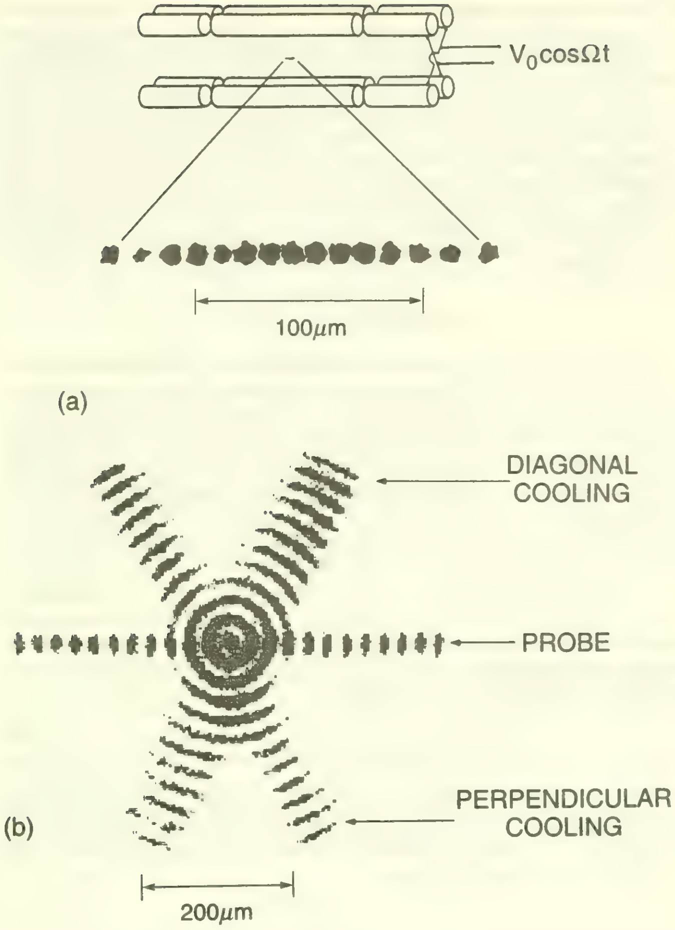

FIGURE 2.1 Correlated behavior observed when small, single-component ion plasmas are laser cooled to temperatures of a few tens of millikelvin. Such laser-cooled ion plasmas are being used to improve the performance of atomic clocks and frequency standards. (a) A crystallized chain of 15 Hg^+ ions, confined in an rf trap by the electrode structure shown. (b) Be^+ ions confined in a Penning trap, imaged by passing three crossed laser beams through the plasma. The bright fringes are the intersections of the laser

ics, including tests of leading theories of atomic structure and crucial tests of extrapolations of previous, lower-Z measurements. Other important experiments include x-ray observations of magnetic octopole decay in atomic spectra; the first use of x-ray polarization to probe the hyperfine interaction in highly charged ions; the first direct measurements of ionization cross sections for highly charged ions; the first measurements of dielectronic recombination cross sections in ions that are important in hot fusion plasmas; the first excitation functions for x-ray lines used in the analysis of high-temperature tokamak and astrophysical plasmas; measurements of line overlaps for x-ray laser design; and measurements of metastable lifetimes in regions of the electromagnetic spectrum inaccessible to other techniques.

Confinement of Antimatter

Trapping techniques similar to those described above for pure electron plasmas have proved to be an efficient way to accumulate and store antimatter particles such as positrons and antiprotons. Single-component positron plasmas, a few cubic centimeters in volume, with a temperature of 300 K and Debye screening lengths less than 1 mm have now been created in the laboratory by accumulating positrons from a radioactive source. Recently, antiprotons from the low-energy antiproton storage ring at CERN in Geneva have been captured by a similar trapping scheme. The antiprotons were cooled to 4 K by collisions with an electron plasma confined in the same cryogenic trap.

These experiments have provided a controlled way to study antimatter interactions with ordinary matter and to study the properties of the antimatter particles themselves. Examples include precision measurements of the mass of the antiproton and positron annihilation phenomena relevant to atomic and molecular physics and to gamma-ray astronomy.

RESEARCH OPPORTUNITIES

Continued progress is expected in the areas identified above in "Recent Advances in Nonneutral Plasmas." In addition, the following topics, while not comprehensive or mutually exclusive, represent important research opportunities in the physics of nonneutral plasmas.

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beams with the plasma's lattice planes, which take the form of approximately spheroidal shells. The plasma rotates about its symmetry axis (normal to the figure), which obscures the image of individual ions within each shell. (Courtesy of J. Bollinger and D. Wineland, National Institute of Standards and Technology, Boulder, Colo.)

Coherent Structures and Vortex Dynamics

The progress made during the past decade in studies of single-component plasmas has created a number of important scientific and technological opportunities. The ability to confine and manipulate pure electron plasmas and to create near-equilibrium states opens up unique opportunities to study transport in plasmas and fluids. The equations that govern two-dimensional flows in these plasmas are identical to the equations that govern two-dimensional flows in inviscid incompressible fluids. Exploiting this analogy, researchers have now conducted sucessful studies of vortex merger in electron plasmas, and the opportunity now exists to study important phenomena in fluid mechanics, such as the relaxation of two-dimensional turbulence (see Plate 2), the interaction of vortices with turbulence and shear flows, and turbulent transport.

Transport Processes

On longer time scales, the transport of particles due to like-particle collisions is only partially understood. In principle, this is a more difficult problem than the interaction of two-dimensional vortices because three-dimensional effects may be important, as well as the combined effects of single-particle and collective interactions. These effects are related to fundamental issues in kinetic theory and transport processes in neutral plasmas, but they can be isolated and studied more easily in single-component plasmas because of the unusually long confinement times.

Confinement Properties in Nonaxisymmetric Geometries

Recently, magnetized, single-component electron plasmas have been created that are *not* symmetric in the plane perpendicular to the confining magnetic field. (See Figure 2.2.) These plasmas were found to have surprisingly long confinement times. It does not appear that these long-lived, asymmetric states can be explained by the simplest models of good confinement of single-component plasmas with cylindrical symmetry, which indicates that the fundamental principles of single-component plasma confinement are not fully understood.

Stochastic Effects

In complex magnetic field geometries, the combined influence of the applied field configuration and the self-electric and self-magnetic fields of the nonneutral electron or ion beam can significantly affect individual particle motion and beam dynamics. For example, this can occur in the periodic wiggler field in free-electron lasers or in the periodic quadrupole focusing field in induction accelerators, particularly at sufficiently high beam intensities. Although the

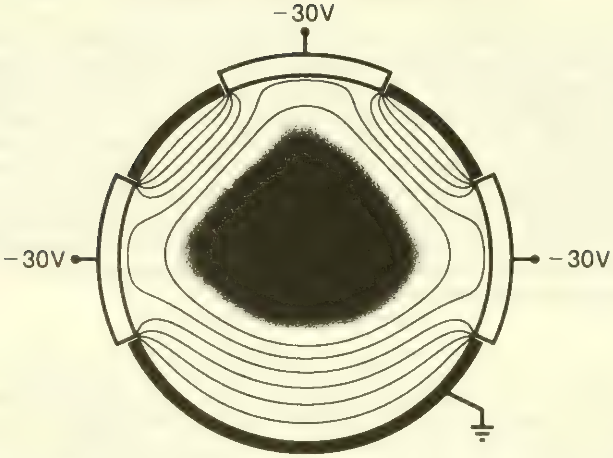

FIGURE 2.2 Shown is the cross section of a magnetized pure-electron plasma confined in a Penning trap. These plasmas were found to exhibit unexpectedly long confinement times. This good confinement is consistent with a recently developed theory that argues that such states are stable equilibria. The plasma is distorted into a triangular shape by the application of electrical potentials (indicated in volts) to sections of a cylindrical electrode structure. The calculated equipotential contours (solid lines) illustrate that the plasma edge follows such a contour. Note that the electrons are closer to the negative electrodes, as expected for a state of maximum electrostatic energy, and as predicted by the theory. (Reprinted, by permission, from J. Notte, A.J. Peurrung, J. Fajans, R. Chu, and J.S. Wurtele, Physical Review Letters 69:3056, 1992. Copyright © 1992 by the American Physical Society.)

particle dynamics are characterized by well-defined single-particle constants of the motion at low beam intensity, where self-field effects are negligibly small, at higher beam intensity the particle orbits can become chaotic and sensitive to the detailed properties of the beam density and current profiles. We do not have a basic understanding of the influence of stochastic effects on the charge homogenization in periodic focusing quadrupole configurations or on the suppression of coherent free-electron-laser emission at high beam intensity.

Strongly Coupled Nonneutral Plasmas

Strongly coupled pure ion plasmas present another set of scientific opportunities. The evolution of the spatial ordering to the body-centered-cubic structure that is expected for a large-volume plasma remains to be studied experimentally. Such experiments will test the predictions of recent theories. Furthermore, for modest-sized plasmas, the theoretical prediction of ordering in concentric spheroidal shells is at variance with observations of open cylindrical shells. This significant discrepancy also remains to be reconciled.

Although there is now a good understanding of many features of the equilibria of ion plasmas and of the linear dynamics of these plasmas about their equilibria, many important questions remain. For example, the transport coefficients (such as the viscosity and the thermal relaxation time) in strongly correlated, magnetized, nonneutral plasma are not understood theoretically. Experiments can measure many of these coefficients. It is likely that future interplay between theory and experiment in this area will be productive in elucidating these fundamental transport processes.

Quantum-Mechanical Effects

The tools now exist to create plasmas in correlated spin states and to create quantized plasmas, in which the quantum-mechanical ground state energy is large compared to the thermal energy in a plasma mode. Another important area for future study relates to the properties of nonneutral plasmas at or near the Brillouin density limit.

Antimatter

The efficient trapping of single-species plasmas has been exploited for the confinement and cooling of both antiprotons and positrons. Further progress in developing efficient techniques for the confinement and manipulation of single-species plasmas will directly benefit studies of antimatter and the interaction of antimatter with ordinary matter. Improvements in the trapping and manipulation of single-component plasmas will lead to the ability to transport antimatter (such as antiprotons) from high-energy accelerators, where they are created, to laboratories throughout the world.

Many important scientific questions can be addressed by collections of antimatter particles confined in traps. For example, one can study the physics of electron-positron plasmas. These plasmas are unusual in the sense that both signs of charge can be highly magnetized, and the "electron-ion" mass ratio is unity. Important physics issues include the nature of confinement and transport in these neutral but highly magnetized plasmas and the nature of fluctuations and turbulence in such equal-mass plasmas. The interaction of low-energy positrons with ordinary matter can also be studied with precision in traps, to address questions relevant to atomic and molecular physics and to gamma-ray astronomy. The 511-keV gamma-ray annihilation line is the strongest astrophysical source of gamma-ray line radiation. Trapped antiprotons will be of use for fundamental physics measurements on the antiprotons themselves. Moreover, addition of a cold positron plasma to the trapped antiprotons is thought to be the simplest way to produce cold antihydrogen, which would be the first creation of neutral antimatter in the laboratory.

OPPORTUNITIES FOR ADVANCES IN TECHNOLOGY

Precision Clocks

A wide range of technological opportunities is likely to result from research on laser-cooled ion plasmas. Spectroscopic interrogation of ions in traps is one of a few possible techniques for developing a new generation of precision clocks. Improvements in clocks using pure ion plasmas hinge on understanding the microscopic distribution function of the ions or, equivalently, on a more quantitative theoretical understanding of strongly coupled plasmas. Improved precision clocks would lead to advances in such diverse areas as navigation and tests of general relativity.

Precision Mass Spectrometry

One of the most important techniques for studying the masses of chemical species is ion cyclotron resonance, where the cyclotron motion of a confined cloud of ions is excited and detected. A large signal-to-noise ratio requires a large number of ions, but in this case precise interpretation of the cyclotron resonance signal hinges on a detailed understanding of the collective modes of oscillation of these multispecies ion plasmas. Scientific issues in this area have only recently begun to be addressed, starting with studies of the cyclotron modes of a single-component electron and ion plasmas, and precision studies of cyclotron resonance for one or a few ions. It is likely that much progress can be made in this area during the next decade.

Ion Sources with Enhanced Brightness

Laser cooling of 1-MeV ions in storage rings has recently been achieved. The development of methods for cooling these single-component plasmas and understanding their behavior can lead to brighter ion beams and hence to enhanced accelerator performance. It is possible that the "sympathetic cooling" of ions confined in a trap with positrons will lead to brighter sources of positrons for advanced accelerators. The successful achievement of cryogenic plasmas opens up the possibility of preparing spin-polarized plasmas. In principle, these plasmas could provide bright sources of polarized particles for use in particle accelerators.

Electron-Beam Ion Traps

Studies of electron-beam ion traps as plasma devices are in their infancy, and progress in this area could lead to performance enhancements by several orders of magnitude. This would enable new kinds of experiments in atomic, nuclear, and surface physics. An enhanced ion source based on the electron-beam ion trap is expected to be useful for surface modification and nanotechnology. In most cases, plasma physics issues are the key to these developments. For example, a thousandfold increase in the x-ray emission rate from an electron-beam ion trap might be achieved by increasing the total electron-beam current, the current density, and the space charge neutralization (i.e., ion density). The total beam current is likely to be limited by instabilities such as those that occur in backward-wave oscillators. The current density is likely to be limited by the brightness of future electron guns, and the (poorly understood) super-emissive, hollow-cathode discharge is a leading candidate for an electron gun. The ion density will be limited by a two-stream instability. The performance of electron-beam ion traps is also limited by discharges and instabilities involving trapped secondary electrons. These phenomena are not understood to the degree necessary to design a reliable next-generation device. Progress has been made only by trial and error. It is possible that the ion output could be enhanced by an even larger amount simply by making the trap longer, but success will again depend on understanding plasma properties of these devices.

A plasma research program in this area might also spin off benefits for other electron-beam devices (e.g., klystrons, traveling-wave tubes, and free-electron lasers) and for other plasma devices (e.g., electron-cyclotron-resonance ion sources and the pure electron or pure ion plasmas described above). In addition to issues related to the electron beam itself, it is known that many electron-beam devices are affected by trapped ions. New types of devices could also evolve from the present experimental configurations of electron-beam ion traps, which, for example, might provide new and inexpensive laboratory sources of x-rays and highly charged ions and microwave devices with trapped ions designed into their operation.

Radiation Sources

The increased understanding of single-component plasmas is likely to have significant impact on the development of beam-type microwave devices, particularly for use in high-power and high-frequency applications. Such applications are discussed in more detail in the section on beams and radiation sources.

Pressure Standard in Ultrahigh-Vacuum Regime

A pure electron plasma confined in a Penning trap can potentially be used to develop a primary pressure standard in the ultrahigh-vacuum regime ($< 10^{-5}$ Pa).

The trapped electrons relax to a well-defined equilibrium state in which the average rotation frequency of the electron plasma is independent of radius. When a neutral gas is present, collisions between electrons and neutrals perturb the plasma and modify the rotation frequency and electron distribution function. By using a reference value for the elastic momentum transfer cross section of the neutrals, the neutral gas density consistent with the observed evolution of the electron plasma can be determined and used to develop a pressure standard.

SUMMARY, CONCLUSIONS, AND RECOMMENDATIONS

In the past two decades, much progress has been made in the understanding of nonneutral and single-component plasmas. New experimental configurations have been discovered and exploited, leading to a better understanding of the underlying physical principles of plasma confinement, approach to equilibrium, and in some cases, mechanisms of plasma transport. There are many potential scientific and technological uses of such plasmas. These opportunities result, at least in part, from the excellent confinement properties that distinguish single-component plasmas from neutral plasmas and enable true thermal equilibrium states to be achieved. Therefore, plasmas with controlled departures from equilibrium also can be created. This allows a study of nonequilibrium plasma phenomena with a degree of precision unachievable in other plasma systems.

Experiments in nonneutral plasmas, such as those described above, can be exploited to address forefront problems in atomic, molecular, and optical physics and in fluid dynamics, as well as in plasma physics. Consequently, it is expected that this will continue to be a vital and productive area in plasma physics research for the foreseeable future. Since these experiments can typically be done with a relatively modest expenditure of resources, they are ideally suited to a university setting.

In addition to the intrinsic scientific value of research in nonneutral plasmas, there are many important applications of these plasmas. Several examples, discussed above and in Chapter 5, "Beams, Accelerators, and Coherent Radiation Sources," include beam-type microwave devices, such as gyrotrons and free-electron lasers, precision clocks and mass spectrometers, and future generations of ion sources.

The progress in this area has benefited greatly by steady support from a dedicated program at the Office of Naval Research and support from the National Science Foundation and the Department of Energy. It is the conclusion of the panel that research on nonneutral plasmas should be considered a vital part of a healthy and vigorous plasma science program in the United States in the next decade. Therefore, the panel recommends that continued strong support be given to research on nonneutral plasmas and to the development of technological applications.

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Inertial Confinement Fusion

INTRODUCTION AND BACKGROUND

The goal of fusion research is to develop a reliable alternative to the present burning of fossil fuels for energy. In the inertial confinement approach to fusion, high-intensity laser or charged-particle beams are used to compress and heat the fusion fuel to the density and temperature required for fusion of the nuclei. The fusion of deuterium and tritium is schematically illustrated in Figure 3.1.

The pursuit of inertial confinement fusion (ICF) depends on many phenomena associated with plasma science. The interaction of radiation with matter in the plasma state and the subsequent energy transport and high-density compression leading to thermonuclear burning of the plasma fuel must be optimally balanced. Nonlinear collective effects must be understood and accommodated. The international goal is to achieve an environmentally improved source of electrical power generation. The majority of the program continues to be implemented in the nuclear weapons laboratories, the Naval Research Laboratory, and the Laboratory for Laser Energetics at the University of Rochester. New large-scale facilities and facility upgrades are currently envisioned with funding authorizations at various stages. Support for the underlying basic plasma science and the breadth of the involved community should be strengthened. The role of basic plasma research within the ICF program may be at a crossroads, requiring timely reexamination.

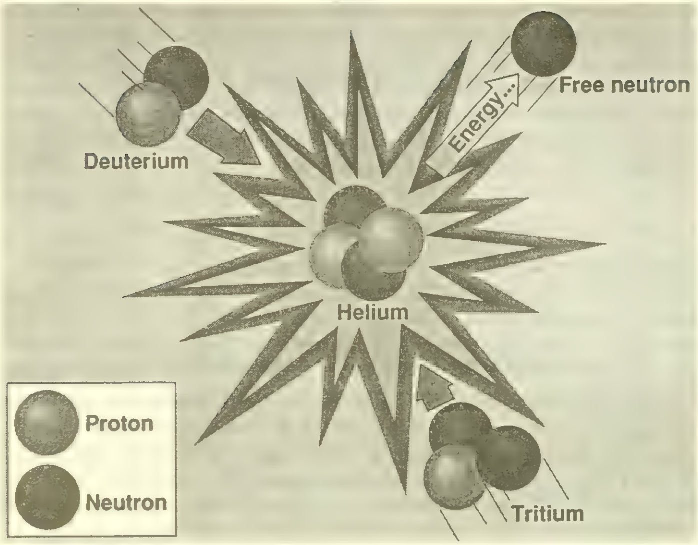

FIGURE 3.1 Schematic diagram of the basic fusion process in which deuterium and tritium nuclei combine to form ^{3}He and a neutron. For electric power applications, the energy from this reaction is transformed into heat and then converted into electrical energy.

RECENT ADVANCES

Laser Fusion

In the past decade, significant progress has been made in the understanding of high-energy-density plasmas created by intense lasers and particle beams. An ICF hohlraum irradiated by the Nova laser is shown in Plate 3. With several notable exceptions, this work has been carried out under the auspices of inertial confinement fusion research with large lasers (i.e., having energies greater than 1 kJ). Direction, progress, and accomplishments within the ICF program have been subject to frequent national review.[1]

Experiments and computer simulations during the past decade have led to a quantitative understanding of the Rayleigh-Taylor instability in hot, ablating plasmas. Future work will examine the transition to turbulence and address Rickmyer-Meshkov and Kelvin-Helmholtz-like instabilities. Two-dimensional hydrodynamic simulations have modeled successfully the linear and early nonlinear evolution of the Rayleigh-Taylor instability in ablating plasmas with a range of initial sources, Attwood numbers, and accelerations. Photon and electron energy deposition lead to finite density gradients and mass removal, which can substantially reduce the Rayleigh-Taylor growth rate from its classical value. Fokker-Planck codes embedded in hydrodynamic simulations have been developed to model better the nonlocal electron energy transport. These simulations describe phenomena such as thermal filamentation and thermal conduction, and influence our basic understanding of the details of the Rayleigh-Taylor instability.

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[1] For example: National Research Council, *Second Review of the Department of Energy's Inertial Confinement Fusion Program*, Final Report, National Academy Press, Washington, D.C., 1990.

Using the Nova laser facility, successful experiments were conducted that addressed the physics of interpenetrating materials at accelerated interfaces (an area of hydrodynamics critical to both ICF and weapons research). Several of the "ignition physics milestones" described in the NRC's 1990 review of the ICF program were achieved, including experimental confirmation of the LASNEX simulation code predictions for Rayleigh-Taylor instability growth rates in the presence of ablation and density gradients for both radiation-driven and electron-conduction-driven planar foils. New diagnostic techniques were demonstrated, including large neutron scintillator arrays; a single-hit scintillator array neutron spectrometer; and a high-energy, ring-aperture x-ray microscope. The Nova target chamber is shown in Figure 3.2.

There has been significant progress in the ability to measure and calculate the radiation properties of complex, partially stripped ions over a wide range of plasma conditions. The recent measurement of iron opacity in dense ($n_e > 10^{20}$ cm^{-3}), warm ($T_e \geq 70$ eV) plasmas in local thermodynamic equilibrium illustrates these advances. These conditions are also relevant for astrophysical plasmas. The demonstration of nickel-like gold plasma x-ray lasers operating at a wavelength of 33 Å is an example of the present capability to model non-LTE plasmas. Sophisticated opacity codes for dense plasmas composed of multi-electron ions have been developed. These codes describe the complex absorption and emission features of these ions in terms of unresolved transition arrays and super-transition arrays, and have led to improvements in modeling radiant energy flow in high-density plasma of interest in both inertial fusion and astrophysical applications. Laboratory experiments have been performed that validate these codes.

Ion-Beam Fusion

An equally robust and successful track record of accomplishments exists for the light- and heavy-ion ICF efforts, which represent alternative "driver" ap-

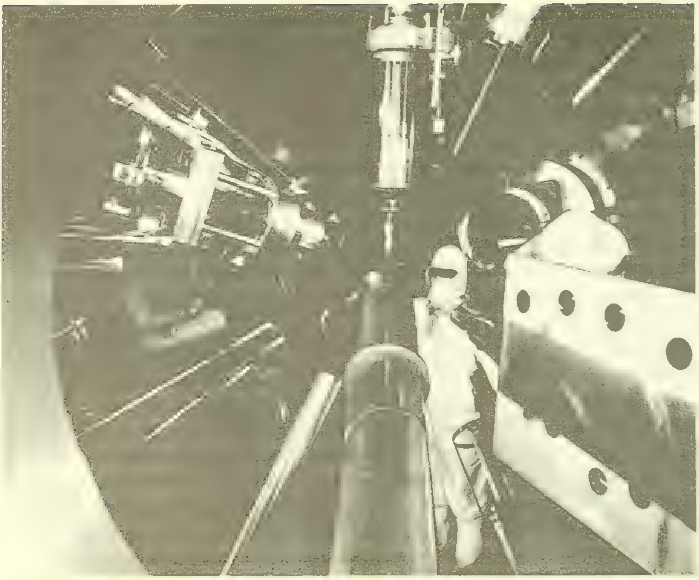

FIGURE 3.2 Photograph of the inside of the Nova 10-beam target chamber at Lawrence Livermore National Laboratory. An essential part of Nova is its diagnostic capability, which includes optical, x-ray, and neutron measurement techniques to study the performance of the ICF targets. These diagnostics surround the target, which is positioned in the center of the chamber on the end of a rod descending from the top of the picture. (Courtesy of Lawrence Livermore National Laboratory.)

proaches, and they are being pursued concurrently with the laser program. A principal aim of heavy-ion fusion accelerator research is to gain an understanding of the dynamics of intense, space-charge-dominated beams in accelerator structures. These beams, which are effectively nonneutral plasmas, exhibit collective behaviors in addition to those bulk motions driven by the externally applied fields. The beam plasma frequency is comparable to the frequency associated with motion in the applied fields. Analytic theory originally predicted a multitude of instabilities; however, most of these were not observed. Computer simulations resolved the disagreement for transverse modes by elucidating the nonlinear behavior and early saturation of the instabilities. Resolution of this disagreement represents a major step in understanding these nonneutral plasmas.

The light-ion ICF program is based on the robust and cost-effective intense ion beam technology developed over the past 20 years. The program has many significant achievements. The ability to transport megajoules of electromagnetic energy at megavolt-per-centimeter electric fields in pulse power accelerators and over several meters of transmission line has been achieved only after careful study of electron insulation in high-voltage devices. Electrons emitted from surfaces are confined by the self-magnetic field of the transverse electromagnetic (TEM) waves, and the behavior of relativistic, diamagnetic electron flow in strong electromagnetic fields is a large extrapolation beyond the physics of magnetrons. The successful generation of multi-kiloampere-per-square-centimeter light-ion beams in magnetically insulated ion diodes has resulted from careful study of the physics of collective effects in electron and ion streams in strong electric fields. This reduction in beam divergence has come from two- and three-dimensional computer codes that model the electromagnetic and particle physics in realistic geometry supported by analytical studies.

Important developments in the physics of the propagation of intense ion beams to the target in an ionized background gas have occurred both experimentally and theoretically. Unique diagnostic systems have been developed capable of measuring ion beams and plasmas on very short time scales and in extremely hostile radiation environments, including the measurement of high ion beam intensities (0.1-1.0 MA/cm^2, 1-12 MV, 1-5 TW/cm^2) using elastic scattering and characteristic x-ray line emission. Diagnostic techniques developed include visible spectroscopy, VUV spectroscopy, ion pinhole cameras, ion movie cameras, Rutherford magnetic spectrographs, and nuclear activation and nuclear track detectors with automatic track counting capability. Stark shift measurements using visible spectroscopy diagnostics on the Particle Beam Fusion Accelerator II (PBFA II) have demonstrated electric fields as high as 9 MV/cm, the largest electric field ever measured using this technique. Fluorescence spectroscopy using dye lasers has emerged as an important technique for measuring the ion-beam divergence in high-powered ion diodes.

SCIENTIFIC AND TECHNOLOGICAL OPPORTUNITIES

New facilities, such as the Omega Upgrade and the National Ignition Facility (shown in Figure 3.3), will enable the creation of plasmas with densities in excess of 10^{26} cm^{-3} and pressures exceeding 200 Gbar. Energy transport (including fusion by-products), the equation of state, and radiative properties can also be studied with these facilities. For example, plasma conditions will be well matched to the study of electron energy transport that is either nonlocal or not dominated by collisions. Configurations can also be established for magnetic fields to play a role. Dense plasmas formed by isochoric heating by penetrating electrons formed with intense short-pulse laser irradiation also may be appropriate for transport studies in which hydrodynamic expansion can be minimized.

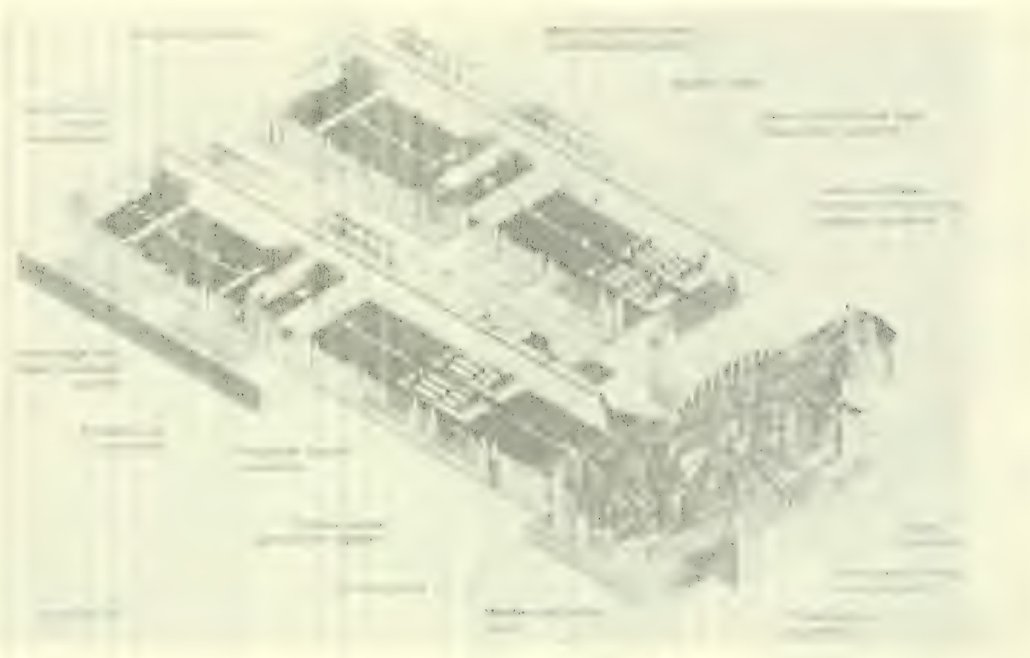

FIGURE 3.3 An artist's rendition of the proposed 192-beam National Ignition Facility (NIF). The NIF is designed to demonstrate an important ICF milestone, ignition and subsequent self-heating of the fusion fuel. Nearly 2 MJ of laser energy (40 times more than the existing Nova facility can produce) will create high-energy-density plasmas relevant to a broad range of research and applications. (Courtesy of Lawrence Livermore National Laboratory.)

These facilities will also enable quantitative experiments that examine hydrodynamic stability properties. Mixing of materials originally separated by interfaces, including the fully turbulent phase and subsequent energy, mass, and momentum transport, will be studied. The development of three-dimensional codes that incorporate turbulent mix models will complement these experiments. A laser-plasma experiment illustrating several relevant multidisciplinary phenomena is illustrated in Figure 3.4.

There will continue to be significant advances in numerical simulations and

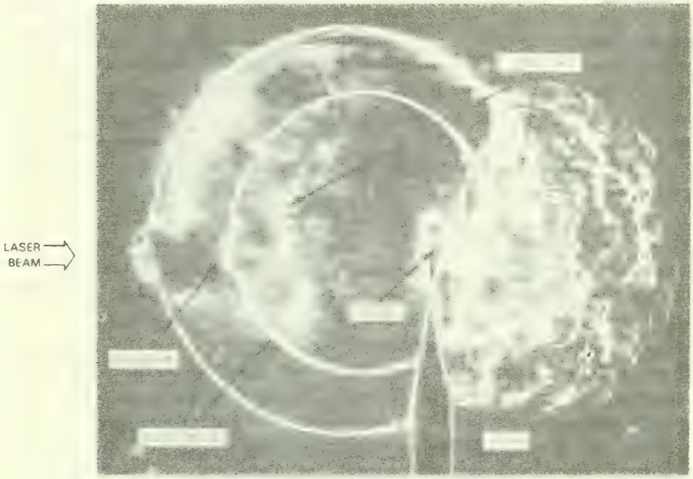

FIGURE 3.4 Images of a laser-produced plasma expanding into a gas. An intense, short-pulse laser (pulse duration 3 ns, wavelength 1.05 μm, irradiance 5×10^{13} W/cm^2) strikes a small target (an aluminum disk 1 mm in diameter and 10 μm thick, mounted on a stalk), creating an energetic, expanding plasma. The plasma initially moves to the left, and accelerated bulk target material propagates to the right. Two superimposed dark-field shadowgrams, which are sensitive to density gradients, image the evolution of a very strong shock wave (Mach number greater than 100) propagating into atmospheric pressure (left side of target stalk) and into a turbulent plasma (right side of target stalk). Such techniques permit physical effects that occur in plasmas with ultrahigh energy densities, such as space plasmas, supernovae, and nuclear explosions, to be readily studied in the laboratory. (Reprinted, by permission, from B.H. Ripin, J. Grun, C.K. Manka, J. Resnick, and H.R. Burris, "Space Physics in the Laboratory," pp. 449-463 in *Nonlinear Space Plasma Physics*, ed. R.Z. Sagdeev, AIP Press, New York, 1993. Copyright © 1993 by the American Institute of Physics.)

the experimental capability to describe the radiative properties of complex, high-energy-density plasmas. The development of non-LTE codes and expansion of the experimentally accessible parameter space will be areas of future research emphasis.

Within the heavy-ion fusion accelerator research area, additional work is needed on longitudinal modes and on the interaction between longitudinal and transverse motions. Because some processes are inherently three-dimensional, advanced three-dimensional simulations need to be developed and applied. The light-ion fusion program provides science opportunities for creating high-density and high-temperature plasmas for materials and plasma science, for developing new computer codes to investigate ion-beam transport in vacuum and plasmas, and for developing a new class of soft x-ray diagnostics that can operate in harsh radiation environments to provide accurate measurements of both Planckian and non-Planckian radiation. Furthermore, the modification of surface properties of materials under bombardment of intense low-energy ion beams can be studied.

Important areas for further research include study of stimulated Raman and Brillouin scattering in laser plasmas at higher intensities, nonlinear interaction of plasma instabilities, plasma opacities and equations of state (EOS), and plasma hydrodynamic stability and mixing. Research objectives within these areas include determining thresholds, saturation levels, and scaling relationships; normalizing high-temperature theoretical opacity models; obtaining high-pressure EOS data and standards for programmatically relevant materials; and characterizing phenomenology associated with plasma compression in excess of a factor of 100.

There also exists a related set of research areas that reflects the interdisciplinary nature of plasma science, when compared with the more formally recognized academic fields of study such as atomic physics, optical physics, condensed matter physics, and fluid mechanics. Consider the basic area of radiation-plasma interaction. ICF programs have used laser light to compress and heat thermonuclear fuel, magnetic fusion energy programs have considered electromagnetic radiation as a supplemental plasma heating source, and programs have been conducted to modify the ionosphere with high-power, high-frequency radiation. All exhibit basic common physical phenomena (such as instabilities, nonlinearity, turbulence, particle acceleration, and heating) and effects of spatial inhomogeneity (such as mode conversion, modification of instabilities, and wave propagation). Phase conjugation and wave mixing in plasmas acting as the nonlinear medium are examples of nonlinear optics phenomena whose analogues in optical media have led to previous technological applications.

The recent development of ultrashort-pulse lasers provides an opportunity to address basic science questions that cross over the descriptive boundaries of ICF plasma physics, atomic physics and condensed matter physics. For example, potential studies include behavior of atoms, ions, and molecules in the strong electromagnetic fields of the ultra-short-pulse laser; the properties of hot, solid-state plasmas; and radiation-matter interactions in all density-temperature regimes.

Early in research on inertial confinement fusion, laser-plasma experiments revealed many interesting plasma physics phenomena, including several types of parametric instabilities, superthermal particle acceleration, and spontaneous magnetic field generation. Ultrafast-pulse laser technology has now progressed to the point where extremely nonlinear phenomena are accessible. Incident electric fields as high as 10 kV/Å transform bound electrons instantly into relativistic free electrons. These so-called tabletop terawatt lasers are university-scale facilities.

Numerous new phenomena will be available for study. These include relativistic self-focusing, relativistic penetration into overdense plasmas, coupling that leads to energetic electrons and ions ($E > 1$ MeV), severe ponderomotive pressure modification of the plasma hydrodynamics, and coherent radiation generation. Large-amplitude plasmons can be created by several different processes (beat wave, wake field, etc.), and their evolution from coherent to chaotic structures can now be studied. Electron heating by these large-amplitude, high-phase-velocity waves can be examined, as well as the subsequent wave-particle interactions.

The short-pulse aspect (<1 ps) also enables studies of high-energy-density plasmas created through rapid energy deposition (~10^{30} W/g may be possible). The pulse duration may also be matched to characteristic time scales of the plasma, such as the period of an electron plasma oscillation for electron densities exceeding 10^{17} cm^{-3}. The fundamental properties of electromagnetic wave interaction change qualitatively, and numerous processes can lead to the acceleration of particles to high energy.

High-irradiance, short-pulse lasers may be able to generate extremely large magnetic fields (B >100 MG) in plasmas. Diagnostic techniques, such as subpicosecond Faraday rotation, hold the promise of measuring the evolution of such fields. Plasma resistivity can be controlled by the judicious choice of experimental parameters (including preformed plasmas), and fields initially containing more than 10% of the laser energy may be possible. Significant plasma flows can be created with high-brightness lasers. These plasma flows, which can have velocities approaching 10^9 cm/s, can be directed across externally applied magnetic fields or used to form counterstreaming plasmas. Numerous plasma phenomena can be studied.

The ultrafast regimes are particularly amenable to computer simulation and are an ideal test bed for improving the understanding of strongly driven plasmas. Research in this area could lead to new compact sources of tunable radiation and to a new class of compact particle accelerators. New diagnostic tools could be made available for material, biological, and electronic applications. Finally, improved understanding of strongly driven plasmas will benefit fusion and astrophysical research, as well as advance nonlinear science.

The equation of state of plasmas formed over a wide range of thermodynamic pathways can be studied at pressures exceeding 1 Gbar. High-irradiance, short-pulse lasers and high-intensity lasers can be utilized in these studies. The creation of strongly coupled plasmas and the near-isentropic compression of plasmas by lasers to densities exceeding 10^{25} cm^{-3} at temperatures less than the Fermi energy are now possible. The properties of such plasmas, of interest to inertial confinement fusion, astrophysics, and condensed matter physics, can now be studied in the laboratory. Experiments have been conducted demonstrating ultrahigh-pressure shocks exceeding 700 Mbar. Pressures readily in excess of 1 Gbar are now possible, enabling the equation of state and other aspects of condensed matter physics to be studied in regimes previously unattainable in the laboratory.

Finally, high-energy-density, non-LTE plasmas play a major role in several multidisciplinary endeavors, including optimized ICF target and driver design, x-ray plasma diagnostic spectroscopy, and x-ray lasers. Energy balance, hydrodynamic behavior, and radiation transfer are all affected by the detailed atomic states and kinetics of the plasma. Experiments would further normalize the computational ability to model and simulate plasma behavior and would further improve the spectroscopic ability to measure and characterize the plasma state.

CONCLUSIONS AND RECOMMENDATIONS

Certain trends are already evident in light of the evolving national priorities triggered by the commonly acknowledged end of the Cold War. First, a healthy process of consolidation and collaboration is evident. Increasingly, joint efforts addressing design, experiment, analysis, and facility issues collectively involve the Livermore, Sandia, Los Alamos, Naval Research, and University of Rochester laboratories. Technology collaboration and transfer arrangements with the industrial sector and a renewed emphasis on the quality and commitment to education are being discussed. Collaborative teaming and partnering among and within government, industrial, and academic organizations are being nurtured all the while in a fiscal environment focused on national deficit and debt reduction. It is in this context that recommendations regarding the funding and implementation of basic plasma science research impacting ICF are made.

A segment of the ICF community holds the view that the health of the field is adversely affected by the priority allocation of resources to facilities and operating costs, at the expense of support for basic plasma research aimed at a fundamental understanding of relevant phenomena. This resource allocation is reflective of an ICF program that relies primarily on computer simulation and full-scale experimental results. It also is suggested that the historical association between the ICF and nuclear weapon programs of the Department of Energy (DOE) has limited the size, nature, and degree of involvement of the basic plasma research community doing related work.

Past classification and facility access policies compounded this problem. However, recent DOE plans to declassify large portions of the ICF program provide a major opportunity to involve the basic plasma research community.

Given budgetary constraints, capital-intensive full-scale experimentation can constrain support for more fundamental theoretical and experimental scaling research and modeling. Although full-scale experimentation is essential, the inclusion of a basic plasma science research component within the fusion energy program can lead to a more timely achievement of the basic goals.

The ability to conduct basic plasma science research in ICF, as described above, depends critically on the accessibility of facilities and the availability of equipment, independent of the organizations and personnel involved. A dual approach is suggested. The previous policy of developing facilities for full-scale experimentation has put in place large numbers of components, subsystems, and equipment. The reconfiguration and recommissioning of smaller-scale research facilities should be considered to make effective use of existing equipment and capabilities.

Consideration should be given to providing the opportunity for a broader representation of participating organizations. Interested universities, small businesses, and corporate America could participate competitively, while at the same time offering cost-sharing opportunities. Existing federal programs, such as the National Laser User Facility (NLUF), the Small Business Innovation Research (SBIR) program, and Cooperative Research and Development Agreements (CRADAs) between industry and the national laboratories, could be helpful in this effort.

Consideration should be given to allocation of funding within the inertial confinement fusion program to support more related basic research and use of major ICF facilities as national user facilities. Where appropriate, ICF facility use should be encouraged in support of nonfusion programs. If no additional funding is available, basic plasma science research judged to be the most important could be funded from large facility accounts.

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Magnetic Confinement Fusion

INTRODUCTION

Plasma science has played a major role in magnetic fusion research from its inception and, in many ways, the quest for controlled fusion has been crucial in the development of modern plasma science. In a fusion reactor, a mixture of deuterium and tritium is ionized and the resulting plasma, which is confined by a magnetic "bottle," is heated to temperatures of the order of a few hundred million degrees centigrade. As illustrated in Figure 3.1, the deuterium and tritium nuclei would fuse upon colliding, thereby forming helium nuclei and very energetic (~14-MeV) neutrons. These neutrons may be captured in a thermalizing blanket and the energy used for electric power generation. The needs of magnetic fusion research required a far better understanding of collective interactions in plasmas than existed in the 1950s and 1960s. After the initial series of experiments, of particular concern was the gross magnetohydrodynamic stability of magnetic confinement configurations, the anomalous transport of energy and particles, and the heating and fueling of confined plasmas to reactor-relevant temperatures and densities. Some of the fundamental properties of collective interactions can be probed in relatively simple plasma configurations, the kind of basic experimental plasma research discussed in Chapter 8. However, many collective phenomena can be observed only in hot and dense plasmas in complex magnetic field geometries. The investigation of such phenomena required the development of new diagnostics to probe the properties of hot and dense plasmas, giving birth to experimental plasma research in fusion grade plasmas. Progress in all of these research areas will be required for ultimate success in controlled magnetic fusion. In the following sections, the panel concentrates on the so-called tokamak confinement concept. A tokamak is a toroidal plasma chamber in which confinement is produced by an axial magnetic field and a toroidal plasma current, usually driven inductively by a transformer. The panel also considers important non-tokamak confinement geometries, however.

MAGNETOHYDRODYNAMICS AND STABILITY

Introduction and Background

To achieve the densities and temperatures required for a successful thermonuclear reactor, a plasma must be contained by magnetic forces (such a confinement geometry is sometimes called a "magnetic bottle") for a sufficiently long time to produce net thermonuclear power. In the attempts to achieve this confinement, stability has emerged as one of the most important problems. A plasma confined by a magnetic field is not in thermodynamic equilibrium and therefore is potentially able to break out of the confinement system by a large variety of instabilities.

Past Achievements

During the past 10 years, great progress has been made in understanding the equilibrium and macroscopic stability properties of the tokamak plasma. The majority of tokamaks today routinely produce equilibria that are much more complicated than those of the original circular-cross-section "doughnut" concept. When a tokamak plasma is deformed from its axisymmetric equilibrium state, macroscopic MHD instabilities usually set in. The most virulent of these are the "ideal" instabilities, which tap the free energy associated with the pressure gradient or the plasma current. The unstable modes grow rapidly and can result in sudden loss of the stored plasma energy. Theoretical predictions of the stability boundaries for MHD modes have been corroborated by experiments. A significant achievement is the validation of the dependence of the beta limit (β is the ratio of plasma kinetic pressure to magnetic pressure) on plasma current, minor radius, and magnetic field. Stable operation regimes have been developed based on a good understanding of the dependence of MHD stability on global plasma parameters. In recent years, MHD research has begun to focus on the next level of understanding, namely, the impact of internal profiles on stability. The so-called second stable regime, for example, is a consequence of the localized high pressure creating a favorable "magnetic well" that stabilizes pressure-driven instabilities. Experiments have already shown that the beta limit can be doubled by optimizing plasma profiles. Furthermore, recent tokamak results have indicated a possible correlation between stability and confinement, offering the promise of an operational regime of enhanced stability and improved confinement.

A second class of important but less virulent MHD instabilities is the resistive instabilities. They allow the magnetic field lines to open up and reconnect and may be accountable for the degradation of confinement.

A third class of MHD instabilities is that driven by energetic particles. Experiments have observed instabilities in beam-heated plasmas that resulted in the ejection of energetic beam particles. Other experiments have used energetic beam ions to simulate the alpha particles in igniting plasma.

Of particular concern are the effects associated with self-generated plasma currents ("bootstrap" currents) owing to high plasma pressures at high temperatures and finite density gradients. High bootstrap current fractions (>50%) have been self-consistently calculated and found to be stable at reasonable values of β_p in major fusion devices. Nevertheless, as the pressure is increased, the plasma may become unstable, and this is observed in some of today's experiments. Once we develop a better understanding of these processes, there are plans to improve the stability at high pressures and high bootstrap current fractions in future tokamak experiments.

Future Prospects

In recent experiments, so-called toroidal Alfvén eigenmodes (TAEs) were driven unstable with neutral beams and high-energy particles driven by radiofrequency power. These TAE instabilities are important since they may be driven unstable by the alpha particles produced in the deuterium-tritium (D-T) fusion reaction. Theoretical calculations of fast particle destabilization thresholds for TAE modes are in reasonable agreement with the experimental results. Upcoming tritium experiments at the Tokamak Fusion Test Reactor (TFTR) in the United States and the Joint European Torus (JET) in Britain will test these models for the first time in tokamaks that have significant densities of alpha particles.

Despite the stability problems as the limits of plasma pressure and currents are approached, significant progress has been achieved by building larger and "smarter" machines. Tokamaks' confinement and stability continue to improve, and it is important to continue to improve the tokamak concept for eventual use as an economical power-producing reactor. The DOE is proposing to build the Tokamak Physics Experiment (TPX), a new "steady-state" national tokamak research facility at the Princeton Plasma Physics Laboratory. This device is now being designed by a national team of physicists and engineers. The plan is to start operation at the beginning of the next decade. One of the TPX's key objectives will be to push the stability limits by controlling the toroidal current profile with current-drive methods (see below).

TOKAMAK TRANSPORT

Introduction and Background

Since plasmas of sufficiently high density and temperature must be formed to accomplish thermonuclear ignition, understanding the transport of energy and particles is the key to the design of a fusion reactor. Virtually all tokamak experiments worldwide have demonstrated a common consistent level of energy and particle confinement in the so-called L-mode (L standing for "low confinement") regime of operation. In addition to L-mode, a variety of regimes have been observed and studied that have improved confinement, typically in response to changes in the plasma boundary conditions. The most ubiquitous of these is the H-mode regime (H standing for "high confinement"), which differs from L-mode primarily by having a transport barrier at the plasma edge. In the H-mode, the energy confinement is typically a factor of two better than in the L-mode. More recently, even better confinement has been achieved in the so-called VH-mode ("very-high") regime, where confinement times up to four times longer than those of L-mode plasmas have been achieved. Characteristics of the H-mode and VH-mode are illustrated in Figure 4.1.

Past Achievements

During the past decade, tokamak plasma parameters comparable with those estimated to be required for fusion reactors have been achieved (Figure 4.2), including maximum central ion temperatures of $T_i \sim 37$ keV (4×10^8 °C), confinement times of $\tau \sim 1$ s, and central densities $n \sim 3 \times 10^{20}$ m^{-3}. These parameters were not all achieved simultaneously, but simultaneous measurement has been achieved of a fusion triple product $n\tau T_i \sim 1.1 \times 10^{21}$ keV s m^{-3}. Current experiments operate with the same dimensionless parameter values as those expected in reactors, with the exception that reactors will require two to three times greater confinement times and electron temperatures than those obtained in today's machines.

Tokamak experiments in the 1980s demonstrated that the transport of thermal plasma energy and momentum across the confining magnetic field is ~100 times faster than predicted by theory. This theory considers only the effects of Coulomb collisions and essentially is an extension of gas-dynamic models to magnetized plasma transport. These observations have shifted the emphasis in tokamak cross-field transport theory from collisional models to ones that include the effects of plasma turbulence.

Dimensionless scaling experiments have begun measuring the scaling of tokamak cross-field thermal transport with respect to the theoretically important dimensionless parameters, in a manner analogous to windtunnel experiments. It has been found that the scaling is unlike that predicted by most theories. In

PLATE 1 Three 82-mm-diameter silicon wafers are shown during plasma processing in a plasma reactor used for the transfer of fine-line features during electronic chip fabrication. The bright regions above the wafers are clouds of fine particles, illuminated by argon laser light. This "dust" is a critical source of defects in chip manufacture, and techniques are now being developed to eliminate it. (Courtesy of G. Selwyn, Los Alamos National Laboratory; work performed at IBM, Yorktown Heights, N.Y.)

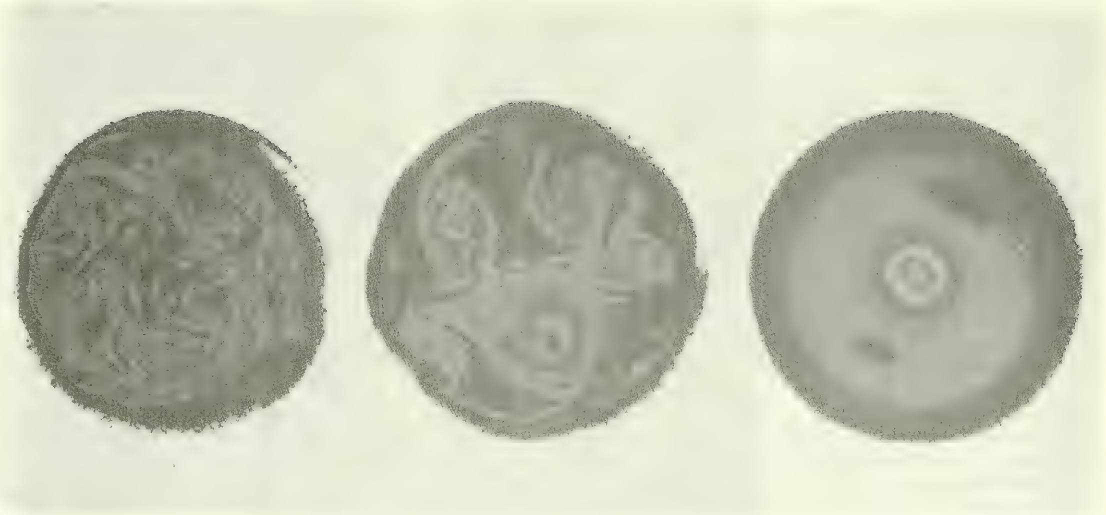


PLATE 2 Relaxation and self-organization of turbulence in a magnetized pure-electron plasma, showing the time evolution of fine-scale vorticity into one large vortex. These electron plasmas are nearly ideal, inviscid, two-dimensional fluids, in which electron density plays the role of vorticity. They permit studies of two-dimensional fluid dynamics not possible in conventional fluid systems. Shown is the evolution of electron density, as time proceeds, from left to right; violet corresponds to high electron density (i.e., vorticity) and red to low density. (Courtesy of C.F. Driscoll, University of California, San Diego.)

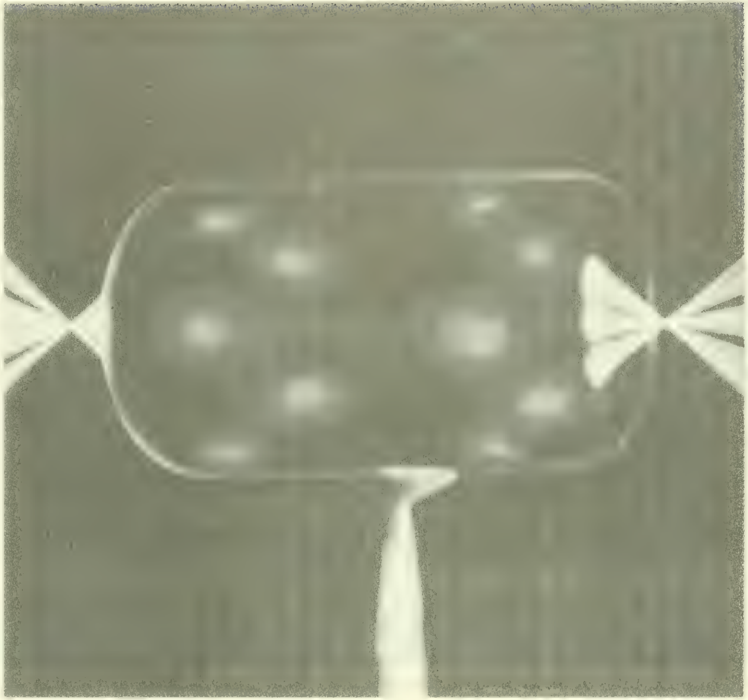


PLATE 3 An ICF hohlraum (a thin tungsten cylinder 2.8 mm long and 1.6 mm in diameter) is illuminated by the 10 beams (in blue) of the Nova laser, which enter through small holes in the hohlraum. In this approach to inertial confinement fusion, intense x-rays with energies of several hundred electron volts are generated and trapped in the hohlraum when the laser beams heat its interior walls. These x-rays uniformly heat the ICF target. In this figure, the laser beams, hohlraum, and stalk are the artist's conception. The orange spots are actual experimentally measured x-ray emission from the hohlraum walls. The laser beams have a wavelength of 0.35 µm. The spots are each approximately 0.7 mm by 0.3 mm. (Courtesy of F. Ze, S. Wilks, and S. Dougherty, Lawrence Livermore National Laboratory.)

PLATE 4 X-ray laser interferogram of a mylar target irradiated with 1.5×10^{14} W/cm^2 by one beam of the Nova laser. To produce this image, an x-ray laser operating at a wavelength of 155 Å, with a pulse duration of 150 ps, was combined with a Mach-Zehnder interferometer consisting of multilayer mirrors and multilayer-coated silicon-nitride beamsplitters. Compared to conventional optical interferometry, the short wavelength of the x-ray laser makes it possible to probe much larger and higher-density plasmas with micron resolution. In this image, one fringe shift corresponds roughly to an electron density of 1.5×10^{20} cm^{-3} for a plasma length of 1 mm. The bright region is self-emission from the laser-heated plasma. (Courtesy of Luiz Da Silva, Lawrence Livermore National Laboratory.)

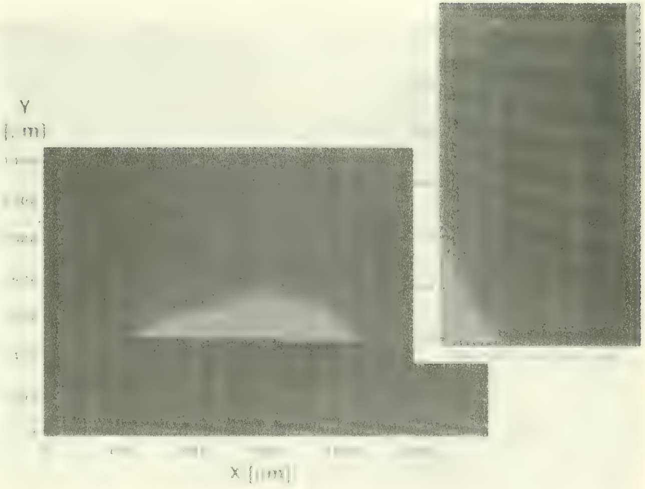
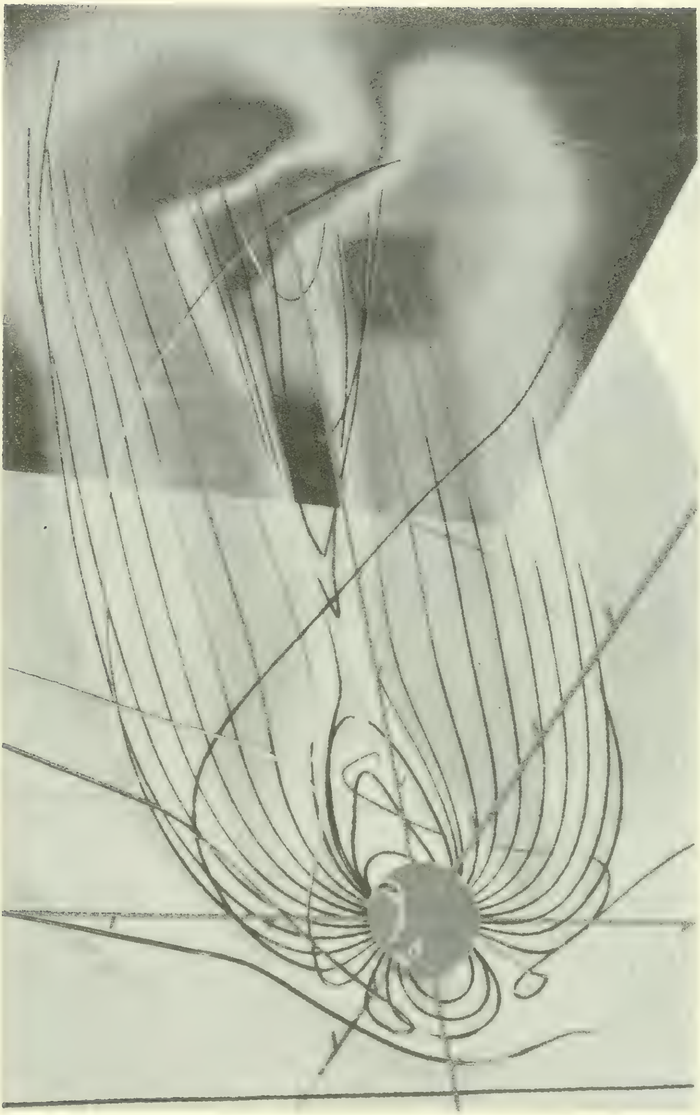


PLATE 5 (facing page below) Computer model of Earth's plasma environment, obtained by solving the single-fluid magnetohydrodynamic equations on a supercomputer. This computation illustrates the breadth of phenomena that can be simulated using contemporary hardware and software. The terrestrial magnetic field lines are compressed and confined on the sunward side by the solar wind, which flows in from the left (i.e., the day side) and is drawn out into a long magnetotail at night. Note the presence both of magnetic field lines connected to Earth and of lines that are completely disconnected, and the juxtaposition of the two at points, both day and night, where magnetic reconnection presumably occurs. Coloring denotes relative plasma pressure, which is high (red) in the interface region with the solar wind (the magnetosheath) and in the near-Earth nightside (the plasma sheet) and is very small (blue) in the distant tail lobes. The size of Earth has been artificially magnified so that the distribution of aurorae, which are proportional to the incident plasma flux, is readily discernible. (Courtesy of J. Raeder, University of California, Los Angeles.)



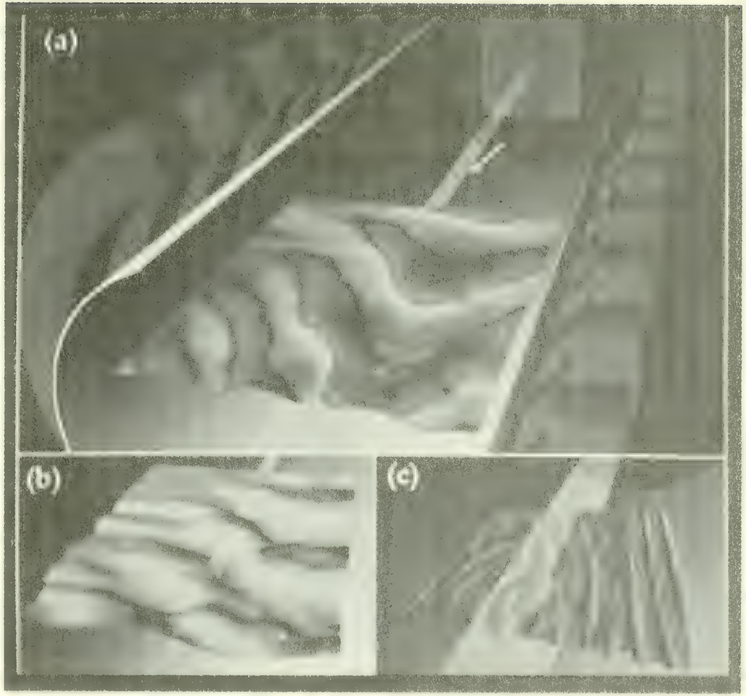


PLATE 6 Laboratory experiment illustrating the nonlinear phenomena resulting from the propagation of whistler waves into a density striation. The auroral ionosphere contains field-aligned density depressions with many scale sizes. The ionospheric plasma has been observed to contain a great deal of whistler-wave activity, as well as ion and electron acceleration and electrostatic waves. In this laboratory experiment, a striation 10 m long and 4 cm in diameter (magenta) is created by non-uniform electron emission from a cathode (orange square). Magnets, which produce a uniform axial magnetic field, are shown surrounding a cutaway view of the chamber. Whistler waves are launched from a loop antenna, which is also shown. (a) Wave magnetic field data (red crests, blue troughs) taken in a plane above the striation agree well with the predicted pattern. (b) Data in a plane containing the striation show a highly distorted pattern. (c) When long perpendicular wavelengths are filtered out, mode-converted, short-wavelength lower-hybrid waves appear. (Reprinted, by permission, from J.F. Bamber, W. Gekelman, and J.E. Maggs, Physical Review Letters 73:2990, 1994. Copyright © 1994 by the American Physical Society.)

PLATE 7 Images of the Sun in white light (left) and in soft x-rays (right) taken by the Yohkoh spacecraft in 1991, illustrating the importance of magnetic fields in solar phenomena. The white light shows that except for active regions, which are distinguished by sunspots, the Sun is essentially featureless at the 5000 K photosphere. However, as evidenced in the x-ray image, the million-degree corona exhibits a great amount of structure due to the influence of the magnetic field. The corona is most active over sunspot regions. At the poles, where the magnetic field is weak and open, x-ray emission is minimal. (Courtesy of Lockheed Palo Alto Research Laboratory, NASA, and the Institute of Space and Astronautical Science of Japan.)

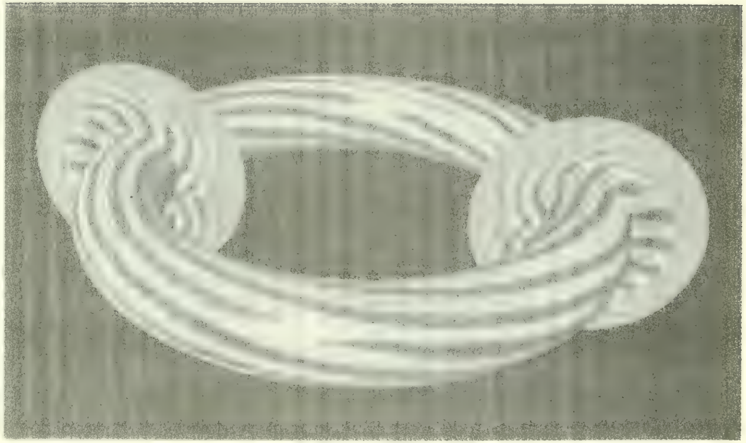


PLATE 8 Computer simulation of plasma turbulence in a tokamak plasma, using a gyrokinetic approximation and 1 million to 8 million particles. Radial and poloidal cross sections of the electrostatic potential are shown during the linear amplification phase. This picture highlights the radial elongation of the mode structure during saturation and the transition to turbulence. The colors represent the value of the potential, varying from positive to negative: magenta, violet, blue, green, yellow, red. (Reprinted, by permission, from S.E. Parker, W.W. Lee, and R.A. Santoro, Physical Review Letters 71:2042, 1993. Copyright © 1993 by the American Physical Society.)

particular, the experiments imply that the size of the turbulent eddies is not always controlled by the electron or ion gyroradius size, but presumably is set by macroscopic scale lengths. New diagnostic techniques used in recent measurements of the turbulent fluctuations within tokamak plasmas have found that the spectrum and implied transport are dominated by moderately long-wavelength modes. Measured changes in the plasma transport are well correlated with changes in the fluctuation amplitude, implicating them as the cause of the transport. In contrast, the measured transport parallel to the magnetic field is in good agreement with the predictions of "neoclassical" theory.

One of the major scientific achievements of tokamak transport and turbulence studies has been the development of a model explaining the formation of the transport barrier in H-mode. This model is based on stabilization of turbulence by sheared $\boldsymbol{E} \times \boldsymbol{B}$ flow (here $\boldsymbol{E}$ is the radial electric field observed in the vicinity of the tokamak edge, and $\boldsymbol{B}$ is the toroidal magnetic field). The measured levels of $\boldsymbol{E} \times \boldsymbol{B}$ flow shear are well above those theoretically required for such stabilization. Furthermore, the increased $\boldsymbol{E} \times \boldsymbol{B}$ flow shear is correlated with the reduction of density fluctuations, cross-field energy, and particle transport both spatially and temporally. These results provide some of the best evidence to date of the close connection between fluctuations and transport. Similar $\boldsymbol{E} \times \boldsymbol{B}$ shear stabilization effects may also take place in the core of tokamak plasmas (e.g., the confinement improvement from H-mode to VH-mode).

The concept of electric field flow shear stabilization of turbulence may be one of the most fundamental contributions of tokamak physics to general fluid dynamics. Although flow shear stabilization can take place in ordinary fluids, a sheared velocity field is usually a source of free energy; hence it usually drives instabilities rather than stabilizing them. Only in a plasma can magnetic shear prevent instabilities driven by velocity shear (e.g., Kelvin-Helmholtz instabilities) so that flow shear can then affect the other instabilities.

Another recent result is the importance of the plasma current density profile in controlling confinement. Experiments modifying the current profile transiently, by inductively driving a skin current, changing the plasma shape, or using external current drive, have shown that, with peaked current profiles, the cross-field transport can be substantially reduced. Although this effect is not understood theoretically, measurements of the local ion thermal transport indicate that it may be reduced by increasing magnetic shear.

Future Prospects

The eventual goal of the magnetic fusion program is the realization of a commercial reactor to generate electricity. Present limitations on confinement and total pressure in the plasma force reactor designers in the direction of multigigawatt units. Understanding and reducing turbulent transport in toka-

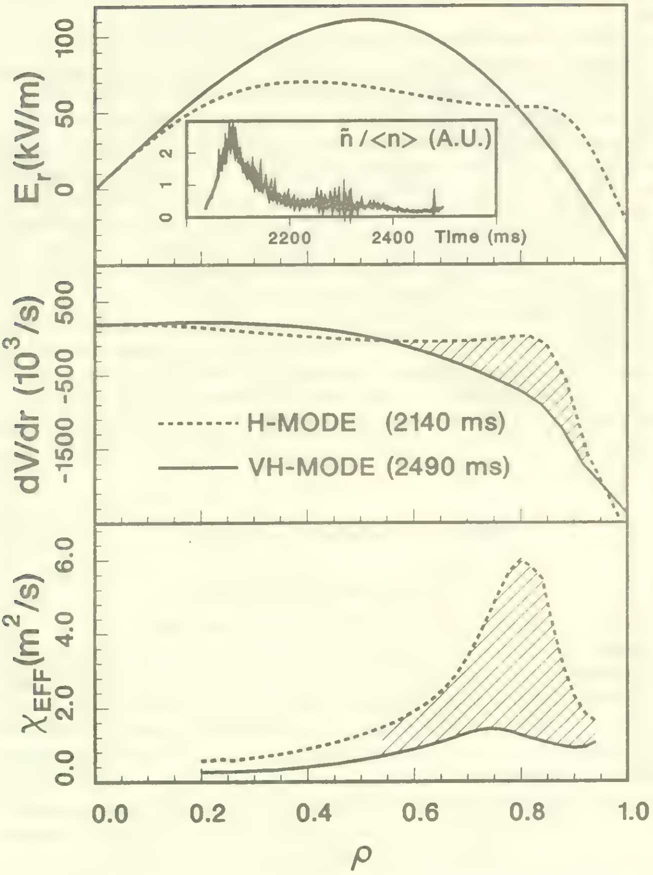


FIGURE 4.1 The loss of thermal energy from magnetically confined plasmas is dominated by turbulent transport. The high-confinement regime (H-mode) in tokamaks is characterized by reduced turbulence at the plasma edge, which inhibits this outward transport of energy. The existence of this regime is believed to be caused by a shear in the drift velocity, associated with a radial electric field. Recently, an even higher confinement regime (the VH-mode) has been discovered, in which the region of high shear and low turbulence extends deeper into the plasma. Shown in this figure are, from top to bottom, the electric field, the velocity shear, and the thermal diffusivity as functions of the nor-

maks could have a major payoff in reduced-size commercial reactors in the next century.

A detailed theoretical understanding of tokamak turbulent transport from first principles continues to elude us. However, continued development and exploitation of novel diagnostics and experiments, coupled with the new nonlinear numerical simulations, should allow identification of the dominant turbulence drive and damping mechanisms during the next decade. Theoretical understanding of the turbulent transport mechanism should allow the development of new techniques for controlling transport to improve tokamak reactors.

Present experiments have shown that control of the plasma current and electric field (flow shear) profiles can have significant effects on turbulence and confinement. Similarly, optimizing the current profile, pressure profile, and plasma shape are important for increasing the beta limit. However, in many cases present experiments in current profile control have been done with techniques that are inherently transient. They demonstrate the principle, but they are not necessarily sufficient to prove that such profiles can be maintained in steady-state or in long-pulse reactors. The challenge for future experiments will be to demonstrate active control of the plasma current, electric field, and pressure profiles by techniques that are economical and applicable to long-pulse or, preferably, steady-state devices.

EDGE AND DIVERTOR PHYSICS

Introduction and Background

Divertors are magnetically separated regions at the boundaries of magnetic confinement fusion devices. Their originally envisioned functions (Spitzer, 1957) were to exhaust heating power and helium "ash" from fusion reactions and to protect the reacting core plasma from impurities. A separatrix is a surface that separates the flux region of the core plasma from the "burial chamber," the region remote from the core plasma where magnetic field lines intercept material surfaces. The plasma that diffuses across the separatrix from the hot core is "scraped off" on those material surfaces, thus giving rise to the name scrapeoff layer. The edge plasma is located just inside the magnetic separatrix and the

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malized minor radius, ρ, for the H- and VH-modes. The inset at the top of the figure shows the time evolution of the normalized density fluctuations, as measured by microwave scattering. (Reprinted, by permission, from K.H. Burrell, T.H. Osborne, R.J. Groebner, and C.L. Rettig, *Proceedings of the 20th European Physical Society Conference on Controlled Fusion and Plasma Physics*, European Physical Society, Geneva, 1993, vol. 17C, part 1, pp. 1-6. Copyright © 1993 by the European Physical Society.)

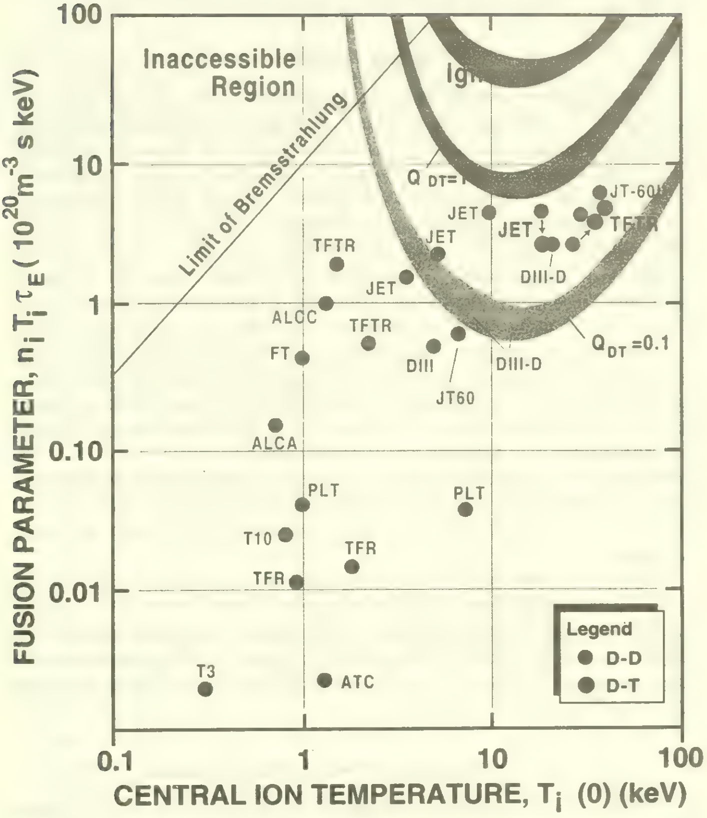

FIGURE 4.2 The approach to power plant conditions of magnetic confinement fusion experiments: The performance of various plasma devices is shown as a function of the "fusion parameter," $n_i T_i \tau_E$, and the central ion temperature, T_i. The boundaries are indicated for Q = 1 in a deuterium-tritium plasma (i.e., "scientific break-even," where fusion power out equals power in) and for ignition.

scrapeoff layer is just outside. The properties of the edge and scrapeoff plasma regions have been shown to exert a profound influence on the confinement and transport properties of the main plasma. This arises through modifications to the boundary conditions on charged and neutral particle and energy flows.

Recent Advances

Research on divertor physics was invigorated in the early 1980s by the discovery of an enhanced core plasma confinement mode (H-mode) for auxiliary-heated plasmas. A doubling of energy confinement was soon verified in many subsequent divertor experiments and, more recently, in limiter-bounded plasmas, as well as in stellarators (an alternate form of toroidal confinement device). New regimes of enhanced confinement were also discovered in ohmically heated tokamaks and in auxiliary-heated tokamaks with special vessel wall treatments. The common link between all these improved regimes of energy confinement was control over hydrogen recycling in the edge plasma.

To understand the transport of energy in the edge region, two-dimensional fluid modeling of the scrapeoff layer has yielded important predictions. A regime of high recycling divertor (HRD) was predicted in which the flux of particles onto the divertor plate greatly exceeded the flow of particles out across the separatrix. This greatly reduces the average energy of the ions hitting the divertor plate and, hence, the impurities generated there by sputtering. A flux enhancement factor of ~20 has been measured in several tokamaks, which confirms the model. In addition, the flux enhancement in the divertor acts as a flow against which impurities created at the divertor surfaces must fight to reach the plasma core region.

Though not directly within the category of plasma edge physics, activities on wall conditioning deserve special recognition. New methods to coat and condition the walls of tokamaks (e.g., carbon, boron, silicon, and lithium coatings and helium discharge conditioning) have resulted in the greatest improvements in core plasma phenomena. At present, this activity is more of an art than a science and is not well understood.

Future Research and Technical Opportunities

The requirements of the International Thermonuclear Experimental Reactor (ITER) program have given new momentum to edge and divertor physics research. The ITER activity, supported by the European Community, Japan, Russia, and the United States, is now in the engineering design phase. ITER will be the first large-scale device built for the purpose of demonstrating controlled thermonuclear ignition and burn as well as the engineering feasibility of a tokamak-based reactor. However, analysis by the ITER team has shown the inability of present conceptual designs of the divertor to handle the intense power loads with sufficient safety margin or lifetime.

Attention has returned to certain old, though still-untested, ideas and recent variations on how to widen the thin power-carrying scrapeoff layer. Ergodization of field lines is under scrutiny. Another scheme relies on the injection of cold gas into the scrapeoff layer to reduce, by charge exchange, the power carried to the divertor plate by plasma ions. Intense radiation, mainly by impurities entrained in the plasma flow toward the divertor plate, may be able to drain the power out of the electron channel. At the extreme, a completely successful embodiment of these approaches would result in volumetric plasma recombination and the replacement of a solid divertor plate by a gas target (an idea that originated in 1974).

The fluid codes presently used to model the scrapeoff layer do not represent a sufficiently accurate description of the physical processes. These must be improved by the direct incorporation of kinetic effects, drift motion, nonambipolar flows, and better atomic physics. With such improvements, these codes could better evaluate helium flows and impurity effects. Comparisons with helium exhaust experiments would prove enlightening. Methods to control helium flows by interactions with waves and MHD activity appear promising. Such activities are under consideration both for U.S. tokamaks and in collaborations on foreign tokamaks.

PLASMA HEATING AND NON-INDUCTIVE CURRENT DRIVE

Neutral-Beam Heating and Current Drive

Introduction and Background

Neutral-beam injection has been the principal method of heating the past several generations of tokamak plasmas and has also found utility in driving toroidal plasma currents. A neutral-beam injector consists of a high-current ion source, with multiaperture grids that electrostatically accelerate ions into a conductance-limiting duct, where a portion of the ions charge-exchange with gas to become fast neutrals. The residual ions are swept out of the beam by a deflection magnet, leaving the high-energy neutrals to pass through a duct across the tokamak's fringing magnetic fields into the plasma. Once inside the plasma, the beam neutrals are ionized, and these high-energy ions are captured by the magnetic field. As they circulate many times around the plasma, they collide with the plasma electrons and ions, transferring energy to them until the beam ions are thermalized. If the neutral beams are injected predominantly in one direction tangential to the plasma's major radius, they can drive net plasma current, reducing the need for inductive current drive after the plasma startup phase.

Past Achievements

Neutral-beam injection has several features that have made it attractive for implementing experiments on numerous tokamaks over the past two decades. Since the beam is trapped by the plasma and transfers energy to it through two-body interactions, the physics involved is relatively straightforward and calculable, allowing the power deposition profile to be predicted accurately. The experimental flexibility has enabled neutral-beam injection to provide most of the heating for transport and confinement studies on tokamaks since the 1970s. Over this period, the injected power levels have been increased from the 100-kW range to 33 MW. Recently, neutral beams have produced ion temperatures up to 44 keV in some of the world's major tokamaks. Beams, including tritium, were used in some cases to produce more than 10 MW of fusion power. (See Figure 4.3.) Beams played an essential role in the discovery of peaked density profile enhanced confinement regimes (called Supershots) and the first demonstration of "bootstrap current," a gradient-driven self-current, on a tokamak. Neutral beams also are important for driving plasmas into H-modes and VH-modes.

Future Prospects

The neutral beams used on tokamaks over the past 20 years have all been based on positive ion sources (with an electron added in the neutralizer to form the neutral beam). The practical neutralization efficiency that can be achieved with positive ions decreases very rapidly at deuterium beam energies of 120 keV and greater. Therefore, for future applications (MeV energies may be desirable in reactors), negative ion beams are more attractive. The achievable neutralization efficiency in an optimized-thickness gas cell is high for negative ion beams (58-60%) and is nearly independent of energy in the hundreds of keV to many MeV range. The roles for neutral beams in the future will include reliable plasma heating and central plasma current drive. Technical opportunities abound for improving the current density, brightness, and gas efficiency of negative ion sources, and for perfecting photodetachment neutralizers and plasma neutralizers that could permit still higher neutralization efficiencies.

Radio-Frequency Heating and Current Drive

Introduction and Background

An alternative way to heat plasmas to high temperatures is by means of radio-frequency waves. Radio-frequency heating spans a very large range of frequencies, from a few megahertz (MHz or 10^6 Hz) to a few hundred gigahertz (100 GHz or 10^{11} Hz). (One hertz designates one cycle per second oscillation frequency.) The low-frequency end corresponds to the regime of Alfvén waves,

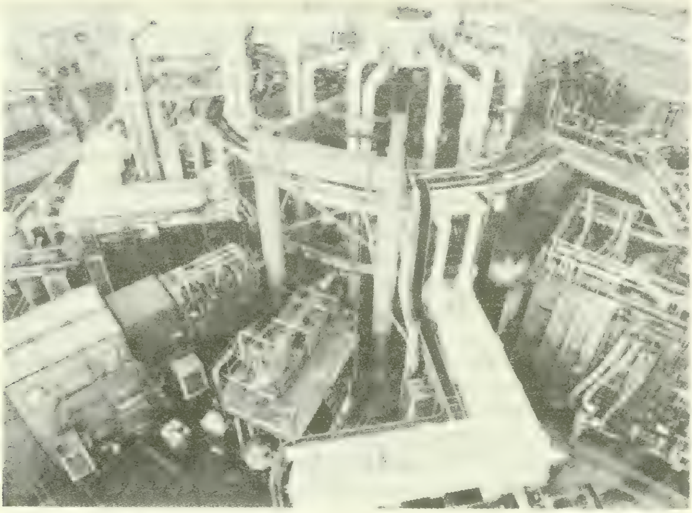

FIGURE 4.3 Photograph of the Tokamak Fusion Test Reactor (TFTR), located at the Princeton Plasma Physics Laboratory. The major radius of the donut-shaped plasma is 2.5 m. Typical plasma currents are 2.5 MA at toroidal magnetic fields of 52 kG. Powered by intense neutral beams and with a deuterium-tritium fuel mixture, TFTR has achieved record ion temperatures of 44 keV and fusion powers of 10.7 MW in second-long pulses. (Courtesy of Princeton Plasma Physics Laboratory.)

while the high-frequency regime corresponds to electron cyclotron waves, which are resonant with electrons gyrating at their gyro (cyclotron) frequency or its harmonics. Other frequencies of interest include the ion-gyro frequency or its harmonics (30-200 MHz) and the ion plasma frequency (more accurately the so-called lower-hybrid frequency) at 1-4 GHz. The basic premise of rf heating is that an antenna installed in the vicinity of the vessel wall radiates electromagnetic waves that deliver rf power from a transmitter to the high-temperature plasma core where the power is absorbed by wave-particle resonances. For example, Alfvén waves and lower-hybrid waves may transfer their energy and momentum to electron motion parallel to the magnetic field by the process of "Landau damping" (named after the famous Russian theoretical physicist who first predicted this kind of resonant wave-particle interaction nearly five decades ago). The accelerated resonant particles eventually dissipate their energy in the background plasma by collisions, thereby heating the bulk plasma particles. If, in addition, the waves travel in a preferred toroidal direction (which can be arranged by proper phasing of the antenna elements), net momentum is transferred to charged particles (usually electrons), thereby generating a net toroidal plasma current. Since rf generators can be operated continuously (cw, or continuous wave operation) at the megawatt power level, a steady-state tokamak operation may become feasible.

In spite of its complexity, the physics of waves in plasmas presents one of the best-understood and most scientifically established disciplines in plasma science. Many aspects of this subject have been verified in fundamental "basic physics" experiments, while some aspects are still under study in the complex field geometry of toroidal configurations. However, the nonlinear aspects of wave propagation still are not well understood.

Past Achievements

Over the past decade, the scientific discipline of radio-frequency heating and current drive in plasmas has seen rapid growth both in the United States and abroad. As in the case of neutral-beam heating, most rf experiments on fusion plasmas in the 1970s consisted of sources amounting to only a few hundred kilowatts, increasing to the ~1-MW level in the early 1980s and culminating recently at the 22-MW injected power level in the ion-cyclotron (ICRF) frequency range. Impressive heating results have been obtained recently in large tokamak devices, where central electron temperatures up to 13 keV have been achieved. In agreement with theoretical projections, directly accelerated "minority ion" species with up to MeV energies have been observed. Detailed energy analysis of these energetic ions shows excellent agreement with "quasilinear" wave-particle interaction theories and large simulation codes, one of the triumphs of modern plasma theory.

Perhaps even more striking results have been obtained in the area of noninductive current drive by rf waves. In this case, the waves not only heat the plasma (i.e:, transfer wave energy to particles) but also transfer net momentum in the toroidal direction. In the past decade, current drive by rf waves has been verified in nearly all frequency regimes. Recently, in Japan, currents at the 3.5-MA level have been driven by multimegawatt lower-hybrid waves. Since these currents are often driven by electrons with energies of 100s of keV, it has been possible to study the transport of these energetic electrons by x-ray imaging techniques. Another scientific spinoff of these experiments is a better understanding of stochastic acceleration of charged particles in electromagnetic wave fields. This may have important application to astrophysical and space plasmas.

Future Prospects

Radio-frequency heating and current drive will likely be used in all future toroidal plasma devices. While ICRF power is eminently suitable for bulk plasma heating, even under reactor-like conditions, electron cyclotron resonance heating may be used either for bulk plasma heating or for special-purpose localized heating (temperature profile control). In principle, localized heating offers the possibility of controlling the pressure profile and thereby improving MHD stability. Lower hybrid waves have been most successful at driving toroidal plasma currents (lower-hybrid current drive, or LHCD). In future experiments, LHCD will be used mainly for driving off-axis plasma currents (current profile control), while the central currents may be driven with neutral beams or with fast magnetosonic waves in the ICRF regime. For purposes of disruption control, highly localized edge current drive with electron cyclotron waves is contemplated.

In the future, rf wave theory will concentrate on the nonlinear regime. In many cases this leads to the study of strongly nonlinear regimes in plasmas, including turbulence, chaos, and stochastic particle acceleration. An understanding of these phenomena will have a large impact on our understanding of similar phenomena in astrophysical and space plasmas, including solar physics.

Radio-frequency heating is a strong technology driver. Higher-power radio-frequency sources are under development in nearly all frequency regimes. In the ICRF regime, new tetrodes have been developed by industry with cw power levels up to 3 MW; future directions include the possible development of 5-MW tube capability. In the lower-hybrid regimes, cw tubes (klystrons) up to 0.5 MW have been developed at 2.45-3.7 GHz, and for future applications, 1 MW tubes are a good prospect for development. Finally, in the electron-cyclotron resonance heating (ECRH) regime, ~1-MW pulsed tubes (gyrotrons) have been developed at frequencies in the 100-GHz range, and future development work promises cw tubes at the 1-MW level at frequencies up to 150 GHz.

DIAGNOSTIC DEVELOPMENT

Introduction and Background

The need to measure detailed plasma parameters in fusion-grade plasma environments has led to many creative applications of plasma science. In turn, numerous advances in plasma science have been inspired directly by the need to understand plasma properties with ever-increasing precision. Important examples from the last decade of research are indicated below. In addition, a summary of future directions is presented.

Past Achievements

Density and electron temperature profiles are routinely measured by laser Thomson scattering and laser interferometry. Electron temperature profiles in hot plasmas are also measured by electron cyclotron emission (ECE). Ion temperatures are often measured by spectroscopic techniques. Among more novel diagnostics, one of the most important and, therefore, most intensely investigated areas is that of incoherent fluctuations and their relationship to energy and particle transport. The fluctuating quantities of interest include density, temperature, and plasma potential. To relate these to transport, it is necessary to measure frequency and wavenumber spectra, along with relative phase, as functions of spatial location. Many techniques have been developed to attack these problems. Beam emission spectroscopy (BES) allows for the measurement of electron density fluctuations by looking spectroscopically at line radiation that results from plasma excitation of high-energy neutral-beam atoms injected into the plasma. BES has led to the discovery of large-scale structures that are now the subject of intense theoretical investigations. Other techniques that have been invented to measure density fluctuations include reflectometry (backscattering from the critical layer), laser scattering, and phase-contrast imaging. Utilizing heavy-ion beam probes of very high energy (~1 MeV), measurements of both density and potential fluctuations have been carried out. Probes have long been used in low-temperature plasmas to measure both density and potential fluctuations, and these are used routinely in the scrape-off region of tokamak plasmas. For the first time, fast scanning probes have allowed access to hotter regions of plasma, inside the last closed flux surface. The ability to measure density and potential fluctuations simultaneously has allowed the first direct measurements of fluctuation-induced energy and particle cross-field transport.

The desire to know the detailed structure of the magnetic field in the hot confinement region of tokamaks has spawned several creative new diagnostic techniques. These include the application of Faraday rotation, Zeeman polarimetry using neutral beams and pellets, and the imaging of the ion clouds that result from pellet ablation. The approach that probably has the most potential for highly precise internal field measurements with good spatial resolution involves an application of the motional Stark effect (MSE), also using an energetic neutral beam. This has led to detailed q profile (safety factor) measurements and new insights into MHD phenomena.

The ability to measure detailed *profiles* of plasma parameters also has matured significantly over the last 10 years. By combining measurements from multiple arrays of soft x-ray sensitive diode detectors with new tomographic inversion algorithms, a wealth of new physics information on the structure and evolutions of fusion plasmas has been gleaned. Charge exchange recombination spectroscopy (CXRS), whereby excited states of hydrogen-like ions of low-Z impurities, such as carbon and oxygen, are populated by charge transfer from atomic hydrogen beam atoms, has enabled detailed local measurements of ion temperature profiles. This is crucial to our attempts to understand the mechanisms responsible for cross-field energy transport. Perhaps even more important, this approach has allowed for the measurement of plasma rotation and, particularly near the edge of plasma, has provided important clues to the relationship between shear in the radial electric field and the transition from the relatively poor L-mode confinement to the significantly improved H-mode confinement regime.

Future Prospects

There is a clear synergism between the need for improved diagnostic capabilities and advances in plasma science that either result from meeting that need or are the means to drive the improvement. Of the many areas that will continue to demand attention in the future, the most important may be the need to develop diagnostics for measuring properties of burning fusion plasmas. Most state-of-the-art diagnostic techniques have to be reexamined when the harsh neutron and radiation environment of a power-producing plasma is considered. With burning plasma, we are faced with the additional task of diagnosing the confinement and slowing down the alpha particles that must ultimately provide the power to sustain an ignited fusion plasma. Although much has already been accomplished in these areas, innovation in the next decade must proceed at a pace at least as rapid as that of the last 10 years. In addition, measurements of other fusion products, as well as stability and confinement of reacting plasmas in the presence of copious amounts of alpha particles, must proceed.

NON-TOKAMAK CONCEPTS

Introduction and Background

Fusion plasma physics has historically evolved through the exploration of a variety of magnetic configurations. The tokamak is the most highly developed concept, and most of the discussion presented above concentrated on this configuration. However, the need for innovative and diverse ideas is as vital as ever in view of the projected multidecade development that lies ahead for fusion.

The main non-tokamak concepts presently under investigation are the stellarator, the reversed-field pinch (RFP), and the compact torus. Each has potential advantages over the tokamak and is a unique source for new plasma physics information. Stellarators have the potential for a steady-state reactor without the need for inductively driven current. Reversed-field pinches, with a relatively weak magnetic field, offer the potential for a compact, high-power-density reactor with normal (non-superconducting) coils. Compact tori offer a reactor geometry in which the plasma torus does not link external conductors. The magnetic mirror approach to fusion is not discussed here since research on this concept ended in the mid 1980s.

The stellarator and reversed-field pinch share the same magnetic topology with the tokamak: toroidally nested, closed magnetic surfaces produced by helical magnetic fields. The relative strengths and origin of the toroidal and poloidal components of the magnetic field distinguish the different approaches. In the stellarator, the magnetic field is produced by current in external windings; in the tokamak, by plasma current for the poloidal field and an external current for the toroidal field; and in the RFP, predominantly by plasma current.

Recent Advances

Microwave and radio-frequency waves are now used to create "true" stellarator plasmas without a net internal current, thus avoiding the problem of "disruptions" that destroy plasma confinement. The absence of a net internal current has allowed detailed demonstrations of the nature of the pressure-driven "bootstrap" current (which must be maximized in tokamaks and minimized in stellarators for optimum performance) through its dependence on magnetic field curvature, as well as its control (e.g., reversing its direction). The plasmas are quiescent with no global instabilities. Operation in the "second stability" regime (in which plasma stability increases with increasing pressure) has been obtained and a connection made with energy confinement. External control has allowed a wide range of magnetic configurations. The similarities in plasma confinement between stellarators with very different magnetic configurations, and between stellarators and tokamaks, suggest that similar mechanisms may be responsible for global transport. The confinement scaling is similar in some tokamaks and stellarators, although stellarators exhibit a more favorable density dependence. The edge fluctuation levels, the corresponding particle transport, and the properties of the edge plasma are similar in both devices. Particle confinement is controlled by the edge properties. It has been increased by using electrically biased limiters and decreased by using magnetic error fields. The improved energy confinement behavior seen in tokamaks is now also seen in stellarators. Initial experiments with biased plates that intercept field lines exiting the plasma (divertors) are encouraging for eventual steady-state particle control in stellarators.

The RFP has evolved significantly during the past decade, both in the understanding of the physics and in the plasma parameters achieved. A fascinating property of the RFP is that it spontaneously generates a portion of its confining magnetic field. This constitutes a laboratory demonstration of the "dynamo" effect, analogous to the astrophysical dynamo responsible for magnetic field generation in stars. A thorough first-principles understanding of the dominant magnetic fluctuations in the RFP has been established through three-dimensional, nonlinear, magnetofluid computations. This theory agrees with experimental observation of fluctuation spectra and nonlinear three-wave coupling. It also offers a detailed explanation of the dynamo mechanism. The equilibrium magnetic field also is understood as a minimum energy state arising from plasma relaxation. These concepts carry over to tokamak phenomena, such as relaxation oscillations and disruptions. Recent attention has turned to the transport resulting from the fluctuations. Techniques have been developed to measure directly the energy and particle transport driven by magnetic fluctuations. As a result of the intrinsically low magnetic field in the RFP, all devices operate at high, reactor-level values of plasma beta. However, the quality of energy confinement, which is typically an order of magnitude worse than that in comparable-size tokamaks, requires improvement and is a topic of continuing research.

Another line of research is experiments with the so-called compact tori, with low aspect ratios. Stable equilibria have been produced in the laboratory, despite the absence of a strong toroidal magnetic field thought to be necessary for stability. This concept has the great advantage of a small unit size, which would significantly lower the cost of an eventual reactor based on this concept.

Future Prospects

Present experiments can develop much of the physics basis needed for improving stellarators, including tests of the contrasting optimization principles for the two main types of stellarators. The largest efforts involve complementary experiments in the United States, Japan, and Germany. Increased plasma heating power will permit fusion-reactor-relevant properties (higher pressure and improved confinement) in long-pulse (30-second) operation, optimization of the stellarator configuration and operational techniques for future large stellarators, and development of steady-state power and particle handling. Large superconducting-coil stellarators now under design and construction will allow true steady-state disruption-free plasmas without the need for externally driven currents or internally driven "bootstrap" currents. The higher heating powers available in these large stellarators will allow studies at higher plasma parameters (pressure, temperature, confinement time) needed for stellarator reactor development.

The evolution of the RFP as a fusion concept requires improvement in energy confinement. From the present experimental database, it is anticipated that transport will be reduced with plasma current. The highest RFP plasma currents operated to date are about 0.6 MA, for durations of less than 0.1 s. Currently, experiments are starting up in Italy that will produce 2-MA plasmas for 0.25 s. This will permit observation of the scaling of confinement on critical parameters, such as the Lundquist number (a measure of the electrical conductivity), which is particularly important for the MHD phenomena prevalent in the RFP. In addition, the evolving understanding of RFP fluctuations and transport is beginning to provide a scientific basis to develop methods to enhance energy confinement. Experiments are beginning in this area.

A major question in compact torus research is whether stable plasmas exist in which the ion radius of gyration about the magnetic field is small. Early experiments possibly were stabilized by the presence of ions with large gyroradii. Compact tori experiments with smaller gyroradii are just beginning.

CONCLUSIONS

The contribution of magnetic fusion research to the field of plasma science has been very significant. Besides being a driver for the development of modern plasma physics, fusion also has benefited greatly from the many advances in basic plasma science. Perhaps the most important area of future research is to learn how to "control" high-temperature plasma in modern confinement devices, which will require learning more about transport and devising effective means of controlling it. This also implies finding stable equilibria at the upper end of the high-beta regimes achieved to date and going beyond present beta limits, especially at high values of β_p (i.e., reduced plasma current in tokamaks). We must learn how to control radial plasma profiles, including those of temperature, density, and current density. At high currents, we must learn how to control disruptions, especially through current profile control. Control is clearly essential for achieving a more attractive fusion reactor based on the tokamak concept. In addition, pursuing other confinement concepts is important, particularly if attempts at control of the tokamak plasma fail or become too complex and expensive. It is also conceivable that a more effective confinement concept than the tokamak could emerge, especially if a steady-state reactor is desired because of technological considerations. However, in the past, funding limitations have often prevented a thorough development of alternate confinement concepts, with the possible exception of the stellerator.

In all confinement concepts, the issue of power and particle exhaust (helium removal) must find a solution in plasma science. This problem is just beginning to be addressed by the scientific community, and its solution will require a thorough theoretical analysis, often involving large codes, and experimental research in the area of "plasma edge" physics. To succeed, this study must include a combination of plasma science, atomic physics, and materials science. Finally, as the next generation of tokamaks enters the thermonuclear regime with burning D-T fuel, the generation of copious amounts of 3.5-MeV alpha particles will open the door to the study of alpha-particle-related plasma phenomena, including stability and transport. New diagnostics may have to be developed to study the interior of the burning plasma environment.

Unfortunately, in the past, many opportunities for fundamental scientific exploration were missed, in some instances because of funding constraints and in others because of changing priorities within the fusion program.

Perhaps the biggest problem in funding more scientific investigations in magnetic fusion is that the level of funding of this fusion program has decreased, in real dollars, during the past decade. Thus, painful choices have often had to be made between upgrading larger facilities to operate in high-performance regimes and increasing the scope of scientific investigations in intermediate-scale devices. Given the mission-oriented mandate of DOE's Office of Fusion Energy (OFE), further research and development will continue to shift toward issues related to burning plasmas, nuclear technology, reactor relevant materials, and so on. At the same time, other funding agencies, such as the National Science Foundation (NSF) and other offices in DOE, have not funded scientific investigations in high-temperature plasmas. If this trend continues, a serious void in the continued growth of high-temperature plasma science will result, despite its scientific merits.

The international fusion community is now engaged in the design of a major fusion facility, the International Thermonuclear Experimental Reactor. This facility will investigate the behavior of burning plasmas under conditions of intense self-heating by alpha particles, and it will demonstrate integration of the nuclear technologies required for a fusion reactor. In addition, the U.S. program has proposed construction of a national facility, the Tokamak Physics Experiment, to investigate modes of continuous operation under advanced performance conditions. The TPX, which is illustrated schematically in Figure 4.4, is planned to begin operation by the end of this decade, when it would become the "workhorse" for research in high-temperature plasma science in the United States.

RECOMMENDATIONS

For the continued development of plasma physics as a scientific discipline it is essential that there be a continued experimental capability to investigate high-temperature plasma phenomena. It is clear that a commitment to increased high-temperature plasma research and training of scientific manpower should be made now. With appropriate funding, the number of graduate students working toward a PhD in plasma-related fields is sufficient to meet such a commitment. The mainstay of this kind of research will remain the DOE Office of Fusion Energy (OFE). However, increased support for energy-relevant basic plasma science by the Office of Basic Energy Sciences (BES) at DOE, in cooperation with the OFE, which is recommended in Part I, would greatly benefit all energy-relevant plasma science and technology. This program could help fund specific experiments on large machines, as well as the operation of small and medium-sized experiments.

Funding at the level of several hundred thousand dollars per year per investigator would be of considerable value to university and industry efforts, even for participation in a large experiment. Initial investment in equipment at the level of a few hundred thousand dollars would also be of additional value. Diagnostic-type experiments could be carried out "piggyback" style at existing facilities.

Many plasma physics problems are best addressed in small- and medium-scale devices. Such devices can be used to test innovative confinement concepts, and the panel sees a need for two to three devices in the United States. In addition, somewhat larger-scale facilities would be desirable to continue basic research in high-temperature (a few keV) plasmas. Such devices might in some

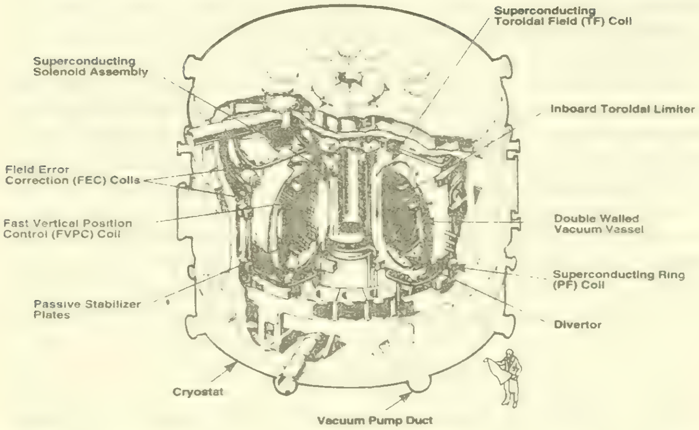
Superconducting Toroidal Field (TF) Coil
Superconducting Solenoid Assembly
Inboard Toroidal Limiter
Field Error Correction (FEC) Coils
Fast Vertical Position Control (FVPC) Coil
Double Walled Vacuum Vessel
Superconducting Ring (PF) Coil
Passive Stabilizer Plates
Divertor
Cryostat
Vacuum Pump Duct

FIGURE 4.4 Schematic diagram of the advanced superconducting Tokamak Physics Experiment (TPX), proposed as a national facility to develop the scientific basis for a compact and continuously operating tokamak fusion reactor. Operating at a toroidal magnetic field of 40 kG and plasma currents of 2 MA, the TPX would investigate modes of enhanced plasma confinement and non-inductive current drive for plasmas lasting longer than 1000 s. (Courtesy of Princeton Plasma Physics Laboratory.)

cases function as user facilities, supported by a consortium of institutions and funding agencies. The panel envisions at least two such devices operating as user facilities. These facilities may be converted from currently operating and/or mothballed devices, most likely tokamaks. Appropriate nonohmic heating and current drive capability should be available on such a device.

Finally, it is also important to maintain a strong parallel program in theory and modeling, for it is the interaction between experiment and theory that facilitates the greatest progress in plasma science. In any case, it is clear that a commitment to continued support of research in high-temperature plasma science should be made now.

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Beams, Accelerators, and Coherent Radiation Sources

INTRODUCTION AND BACKGROUND

Consideration of the state and health of plasma science within the grouped disciplines of intense charged-particle beams, accelerators, and coherent radiation sources presents a picture perhaps representative of trends relevant to plasma science in general. Recent history suggests themes, several of which appear in common with other areas impacted directly or indirectly by plasma science. Basic and applied research have been supported indirectly within large Department of Defense (DOD) weapons-driven and DOE energy-driven application programs, such as the directed-energy weapons, nuclear weapon effects testing, and magnetic/inertial confinement fusion. There have been notable scientific and technical accomplishments in this area, along with visible examples of yet to be achieved or inflated expectations. Finally, and perhaps most important, there is concern about future funding availability and the organizational advocacy necessary to sustain, and advance, the underlying intellectual, facility and equipment infrastructure in light of evolving national defense, economic, and social priorities.

RECENT ADVANCES AND SCIENCE AND TECHNOLOGY OPPORTUNITIES

Intense Charged-Particle Beams

The mission for intense charged-particle beams has changed considerably over the last decade. Research and development sponsored by DOD, the Strategic Defense Initiative Organization (SDIO), and DOE resulted in facilities such as the Advanced Test Accelerator (ATA) at Lawrence Livermore National Laboratory for directed-energy weaponry, low-impedance multi-terawatt pulsed power machines for nuclear weapon effects simulation, and intense beams for fusion plasma heating. Kiloamp-MeV electron beams were developed that support high average power operation in excess of 100 kW with repetition rates approaching 1000 pulses per second (pps). Gyrotrons, devices that utilize a spinning electron beam in an axial magnetic field to produce millimeter waves for electron cyclotron resonance heating, successfully generated several hundred kilowatts in long pulses up to 3 s in duration at frequencies up to 140 GHz. Considering that 10 years ago, 100-ms outputs at 28 GHz and comparable power levels were representative, the technical community is justifiably proud of this technological accomplishment. Similarly, klystron technology has been advanced to higher frequencies (11.4 GHz) and powers (up to 100 MW), and the operation of a gyroklystron amplifier at the 20-MW power level at 11 GHz has been demonstrated.

Many of the military mission-oriented efforts have been canceled. However, industrial applications of high-energy electron beams, including bulk sterilization of medical products and food, toxic waste destruction via oxidation, and processing of advanced materials, are in the demonstration stage. Technology transfer from the laboratories to industry is being encouraged actively. Having invested several hundred million dollars over the past decade in developing intense charged-particle-beam systems for military use, the emphasis by federal agencies on technology transfer for industrial applications seems prudent. Charged-particle-beam parameters vary greatly, depending on the application. A NASA concept for beaming power to space requires basic plasma science research addressing such physics issues as low emittance growth ($< 20\pi$ mm-mr), beam breakup modes, and high current beam transport. Similarly, high-energy electron-beam systems proposed for toxic waste cleanup, enhanced welding, heat treatment, and material processing generally have less stringent requirements on voltage flatness and emittance, but require reliable generation and maintenance of very high average powers.

The interaction of intense charged-particle beams with plasmas, partially ionized gases, and matter offers rich scope for the study of strongly driven collective processes complementary to intense laser-plasma interactions. Electron and ion sources for intense beams have progressed from an empirical art to a developing science. Experiments, simulation, and analytical theory have contributed to this evolution, stimulated by the needs of inertial confinement fusion and other research programs.

Intense ion beams also make possible the creation of magnetic field-reversed ion rings in which the self-magnetic field of the circulating ion current exceeds the externally applied magnetic field. Such a ring would be a compact object of high energy density with unique theoretically predicted features, such as low-frequency stability. If these predictions are borne out experimentally, there may be many uses for such rings, including the possibility of ion acceleration to high energy for various applications. A state-of-the-art electron and ion accelerator is pictured in Figure 5.1.

Accelerators

Several new techniques have been demonstrated that can produce large-amplitude, coherent, high-phase-velocity electron plasma waves. These include the beat wave and wake field concepts. An electron beam accelerated by a beat wave accelerator is pictured in Figure 5.2. Such beat wave accelerators have achieved accelerated electron energies of 9 MeV within distances of 1.5 cm. Attaining 500-MeV energy gains at GeV-per-meter rates is considered a plausible goal within a five-year period in which progress, funding priority, and follow-on application potential may be assessed.

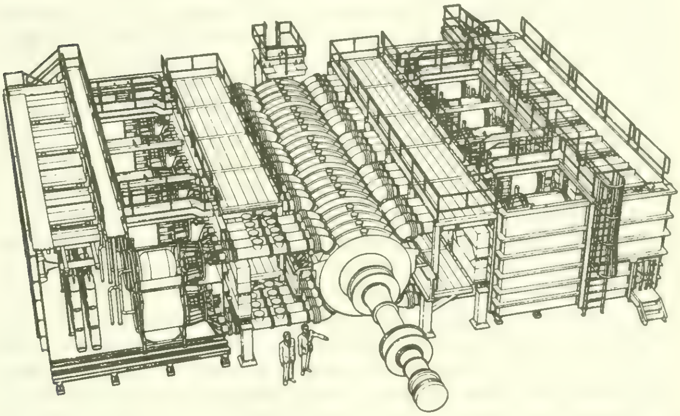

FIGURE 5.1 Schematic diagram of Hermes III, a 16-TW ion and electron accelerator that became operational in 1988. It represents a new class of accelerators that combine state-of-the-art pulsed power designs with high-power linear-induction accelerator cells and voltage addition along an extended magnetically self-insulated vacuum transmission line. This technology is particularly suited for applications requiring high output voltages (tens of megavolts), with megampere-level currents and short pulse widths (e.g., as small as tens of nanoseconds). Hermes III is used in its negative polarity configuration to generate an electron beam of ~20 MeV and 700 kA. It can also be configured in positive polarity to drive an ion beam diode. (Courtesy of J. Ramirez, Sandia National Laboratories.)

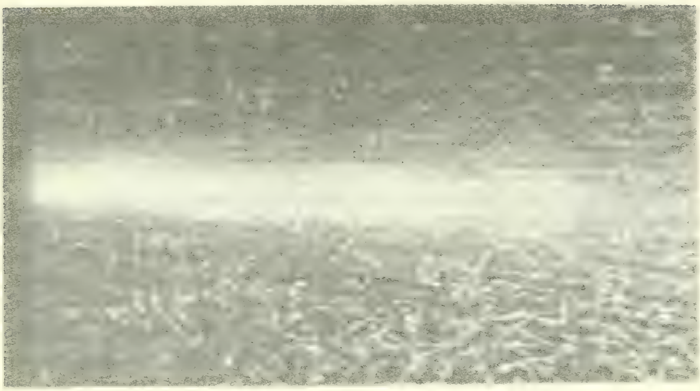

FIGURE 5.2 In a plasma beat-wave accelerator, a pair of laser beams fired into a dense plasma excite a high-phase-velocity plasma wave, and the electric field of this wave accelerates an externally injected electron beam. Shown is an electron beam that has been accelerated to more than 5 MeV in less than 1 cm in such a beat-wave accelerator. This technique holds promise for developing miniaturized particle accelerators for research, medicine, and industry. (Courtesy of C. Joshi, University of California, Los Angeles.)

These present and future very-high-energy, low-emittance, short-pulse electron beams should also further enable progress in other accelerator schemes, such as the plasma wake field accelerator. Relatedly, scaling principles for focusing electron and positron beams using thin plasma slabs as plasma lenses have recently been demonstrated, with 600-μm focal spot sizes achieved. In this case, basic plasma science is being exploited to develop an important "component" of an accelerator system rather than the entire system itself.

Relativistic 2 1/2-dimensional particle-in-cell codes, developed for inertial confinement fusion research, are now being employed to study the physics of short-pulse, ultrahigh-intensity laser-plasma interactions. Phenomena including severe hydrodynamic distortion by the intense light pressure, heating of electrons and ions to ultrahigh energies, relativistic penetration to supercritical densities, and relativistic self-focusing have been observed.

Other novel applications have also been developed, including frequency upshifting of electromagnetic radiation by reflection from ionization fronts and the generation of picosecond pulses of x-rays by irradiation of dense plasmas with ultrashort pulses. The wide-ranging progress may lead to compact particle accelerators, compact sources of tunable radiation, and new diagnostic tools for materials and biological applications.

DOE is supporting the development and operation of state-of-the-art "user test facilities" available to researchers in the field. Examples are the Accelerator Test Facility at Brookhaven National Laboratory and the wake field test facility at Argonne National Laboratory. On the other hand, opinions exist within the community that per-grant funding has not kept pace with what is now perceived to be required for conducting experimental plasma physics research, even with access to state-of-the-art facilities. This situation appears to be compounded further by the absence of any clearly identified "organizational champion" within NSF, DOE, or DOD.

Coherent Radiation Sources

As discussed above in Chapter 2, nonneutral plasmas, such as intense charged-particle beams, exhibit a wide range of collective phenomena. Some collective instabilities limit the performance of accelerators and storage rings and must be minimized; others can be used to convert beam kinetic energy into coherent radiation. New-generation coherent sources, which use electron beams and are based on beam instabilities, operate from the microwave range to the millimeter, infrared, visible and ultraviolet regimes, with previously unattainable intensities. The most prominent of these new systems is the free-electron laser (FEL). Other configurations include gyrotrons and cyclotron masers, and a variety of Cerenkov devices. The basic principle underlying these devices is electron bunching stimulated by an ambient, co-propagating electromagnetic wave. In a properly prepared system, the electrons of the beam, initially distributed at random, can be made to form clusters or bunches. If the bunch dimensions are comparable to or smaller than the wavelength of the desired radiation, coherent emission ensues. Thus, the principle of bunching is somewhat analogous to stimulated emission in conventional lasers.

Free-electron lasers have several important and distinctive features. The oscillation wavelength is not constrained to fixed transitions as in a conventional laser, thus allowing broad tunability. The pulse length is determined primarily by the electron beam, so that rf accelerators can be used without much difficulty to make picosecond pulses. Electron beams can transport high peak and high average power, making the FEL, with its reasonable conversion efficiency, a potentially attractive source of high-power radiation. Because there is no medium except the beam, problems associated with absorption and scattering can be avoided.

Over the last decade, pioneering studies have been carried out concerning the physics of the relevant nonlinear electron-wave interactions that govern the processes in these free-electron radiation generators. Concurrently, significant SDIO investment was made in free-electron laser R&D as a strategic missile defense system. Experimental, theoretical, and computational studies addressed relevant nonlinear interactions such as trapping and sidebands, mode-locking and phase stability, three-dimensional effects, time-dependent phenomena, and high-efficiency operation. High power (gigawatts) and high efficiency (40%) were demonstrated at the longer wavelengths. Systems have lased using storage rings, linear rf, induction and electrostatic accelerators, microtrons, and low-energy beams. However, the accomplishments within the strategic missile defense arena fell short of what was promised and expected, potentially leaving a blemish on an otherwise promising technology.

Most of the basic experiments referred to above were conducted on accelerators built for applications other than the free-electron laser. The SDIO sponsored several small-scale "user facilities." For the free-electron laser to find its appropriate place among coherent radiation sources, research is required to gain a detailed theoretical and experimental understanding of the temporal and spectral properties, to extend operation to shorter wavelengths in the VUV regime, and to increase the efficiency at the shorter wavelengths. A collaboration among academic institutions, national laboratories, and industrial organizations in the design and construction of a next generation of user facilities and the pursuit of the ensuing research would seem appropriate, given that the advocates and potential users of this coherent radiation source technology are successful in establishing its relative priority.

Coherent radiation source research in the x-ray portion of the electromagnetic spectrum includes synchrotrons/undulators, x-ray lasers, and harmonically converted short-pulse optical lasers. Of these three, major support has been given to the synchrotron/undulator effort (i.e., the Advanced Light Source at Lawrence Berkeley Laboratory and the Advanced Photon Source at Argonne National Laboratory). Brookhaven has an active users program dedicated to many areas of research, including biology, materials science, basic atomic physics and chemistry, and semiconductor physics. Much of the x-ray laser research work to date has been carried out with internal research and development funds at national laboratories. This review did not address the x-ray laser research conducted under the auspices of the Strategic Defense Initiative Office.

Concepts for soft x-ray lasers were developed successfully and demonstrated in the laboratory during the past decade. The generation of a dense, hot plasma by laser irradiation was a key feature of this success. This progress was the product of close collaboration among plasma theory, atomic physics, and laser science. Since the initial work, x-ray lasers at more than 50 different wavelengths have been demonstrated in about 10 laboratories worldwide. These x-ray lasers have been demonstrated with wavelengths from 400 to 35 Å, output powers to 100 MW, brightnesses eight orders of magnitude graeter than those of undulators, bandwidths of 5×10^{-5} at full power, and near-diffraction-limited and partially coherent output beam characteristics. Short-pulse harmonically produced x-rays are currently in the demonstration phase with wavelengths approaching 100 Å having been generated, albeit at low ($< 10^{-9}$) efficiency.

At present, synchrotrons have become undulators offering high coherent average power, but at great cost. The recent development of high-average-power glass lasers promises a high-average-power x-ray laser alternative to synchrotron sources at substantially reduced costs. Sources are envisioned with wavelengths between 100 and 150 Å and bandwidths of 5×10^{-5}, producing 10^{15} photons per pulse at 1 Hz, that will occupy physical footprints approximately 3 m by 6 m.

X-ray laser researchers aim at achieving shorter and shorter wavelengths and high coherent output energy. Exploiting ultrashort, subpicosecond laser pulses, photoionization pumping schemes render plausible lasing at or near 14 Å within 5 to 10 years. Harmonically generated x-rays from ultrashort-pulse laser-gas jet interactions are striving to achieve high efficiency at short wavelengths to go along with their inherent tunability and coherence. In turn, these sources can be envisioned to serve as drivers for further wavelength down-conversion providing improved radiation sources in the spectral regime of the order of tens of keV, which would significantly broaden potential medical and industrial applications.

The features of high power, narrow bandwidth, short pulse, and coherence make x-ray lasers attractive for future applications, such as biological microimaging, photoelectron spectroscopy, and probing of dense plasmas. (See Plate 4.) Given these scientific successes and potential applications and societal benefits, it is a serious deficiency that no federal agency has taken on the mandate to support x-ray laser research.

Advances in this area are hampered by the presence of large capital-intensive synchrotron and inertial confinement fusion facilities that historically have either siphoned off the majority of potential funding support or programmatically relegated the research to a piggyback status. The multidisciplinary nature of the research complicates the effort to accrue the critical mass of funding that would support a robust program.

CONCLUSIONS AND RECOMMENDATIONS

Research and development on particle beams, accelerators, and coherent radiation sources offers a wide range of opportunities for technological advances of importance for our society. Examples include food sterilization; waste treatment; welding and materials processing; advanced accelerator development; and the development of new, intense radiation sources for a wide range of applications. In the past, much of the basic science and development in this area was driven by military applications. However, given recent changes in emphasis on military needs, there is a danger that opportunities will be lost unless research and development continue to be pursued in areas in which there are significant technological opportunities. As discussed elsewhere in this report, there is a general need for support for small-scale basic research. This also is true in the areas of beams, accelerators, and radiation sources.

Given these considerations, the panel recommends the following:

1. Dual-use opportunities of defense-driven technologies should continue to be pursued.
2. Existing hardware and facilities no longer needed for ICF and SDIO applications should be made available for other scientific and technological applications.
3. Support should be given to small-scale basic research in these areas.
4. Particular attention should be given to the development of advanced concepts of particle accelerators and of new, intense radiation sources, such as x-ray lasers.
5. Where practical, the use of large facilities by outside users to pursue the scientific and technological goals described in this section should be encouraged.

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Space Plasmas

INTRODUCTION

Background

Space plasma physics is the study of natural plasmas in the solar system and associated technological applications. It is concerned with the ionospheric and magnetospheric plasmas of Earth and the other planets; the physics of the solar plasma internal to the Sun, the solar corona, and the solar wind; the interaction of the solar wind with planets, asteroids, dust, comets, and eventually the interstellar medium; and technology applications ranging from electric propulsion to space "weather" predictions. This is a vast, multiscale, physical domain wherein there are large variations in plasma sources, average thermal energy, flow velocities, magnetic field strength, and other physical parameters. The results are a rich collection of plasma physical behaviors that provide important intellectual challenges to fully understand the underlying processes. It is important to consider the wide variations in physical conditions in terms of spatial dimensions. The largest is that of magnetohydrodynamic flow phenomena. These include the massive outflow of solar plasma as the solar wind, which, entwined with the solar magnetic field, sweeps outward past the planets to its mixing with the interstellar medium. On a smaller scale, within planetary atmospheres, more complicated plasma flows are driven by electromagnetic fields associated with the interaction of the solar wind with the planetary body and atmospheric heating caused by solar radiation. On an even smaller scale, microscopic physical processes are occurring within plasmas in space. For example, the selective acceleration of electrons and ions in the high-latitude, high-altitude regions of Earth leads to the formation of highly structured streams of energetic charged particles that impact Earth's upper atmosphere, creating the aurora. Finally plasma technology devices such as electric propulsion and plasma contactors operate on a small size scale, establishing their own local boundary conditions and interacting with the nearby space plasma.

Knowledge of the physical processes operative in all of these examples is an important goal of space plasma physics for several reasons. First, it provides us with an understanding in quantitative terms of the variety of interrelated complex processes acting to shape and influence our terrestrial environment. Second, parts of the space plasma environment may be prototypical of the astrophysical environment. Third, space phenomena lead to fundamental scientific questions relating to the behavior of plasmas under conditions that can be very different from those created and studied in terrestrial laboratories. Finally, knowledge of the science underscores the development of technological applications operating in or based on the space plasma environment. As a consequence, investigations of natural space plasma processes extend the frontiers of human knowledge, enabling broader physical understanding of plasmas within the context of their general behavior.

Understanding Earth's plasma environment also has important practical consequences. Among these are an ability to model and predict ionospheric, magnetospheric, and interplanetary disturbances that could adversely affect ground-based communications, sensitive instrumentation in geosynchronous orbit, and the safety of astronauts participating in future interplanetary endeavors.

Status

The era of in situ exploration of space plasma physics began in 1946 with V-2 rocket "snapshots" of the terrestrial space environment and continues aggressively today. Measurement techniques include both direct sampling and space-based remote sensing. An excellent example of the latter is the global observation from space of aurora at UV and optical wavelengths, clearly delineating the dynamics of the auroral oval. The initial exploration of the terrestrial magnetosphere and ionosphere is now reasonably well complete, although there are still regions of the solar system that have not yet been explored at all (e.g., Pluto, the heliopause, the solar corona) and regions that have been seen only through brief flybys (e.g., Mercury, Uranus, Neptune). Emphasis now is shifting to the details of physical processes controlling these plasmas. The results of all modern theories and models have depended significantly on the progress of in situ observations.

Ground-based remote sensing studies of space plasma physics have played an important role by providing long-term, localized observations and understanding. Incoherent and coherent radar observations of natural ionospheric phenomena provide information with good temporal and altitude resolution from a few physical locations, thus complementing spacecraft measurements that give good global coverage. Magnetic and optical observatories help in elucidating global current systems and local energy deposition rates. Ground-based measurements that "modify" the natural plasma in the ionosphere have provided information on the physics of a variety of plasma instabilities and related phenomena.

Over the past two decades, our capability to numerically investigate the behavior of space plasmas has steadily improved. Models and simulations of the 1960s and 1970s evolved as a consequence of attempts to understand particular features of the solar-terrestrial environment (e.g., the composition and thermal structure of the atmosphere and ionosphere, the dynamics of interhemispheric plasma interchange, the coupled dynamics of energetic plasma in the magnetosphere and electric fields and currents in the magnetosphere and ionosphere, the interaction of the solar wind with the geomagnetic field, the formation of shocks in the solar wind, the propagation of solar and galactic cosmic rays in the solar wind, and the dynamics of magnetic reconnection). More recently, the ability to study plasmas on a microscopic scale has evolved through the use of various simulation techniques with supercomputers. These codes permit the investigation of various modes of plasma dynamics associated with internal energy and momentum transfer between the plasma constituents and plasma waves. Unfortunately, owing to limitations of computer resources, these studies are often limited in terms of their spatial and temporal resolutions.

The last 35 years of satellite exploration and ground-based experiments, going hand in hand with theoretical modeling and simulation, have put us at a stage where the gross plasma morphology of the solar system is defined in an average sense. This large-scale picture is a synthesis of a relatively few observations that are localized and scattered in both space and time. The major task ahead in our studies of space plasma physics is to obtain the necessary information to be able to understand and elucidate the processes that control the behavior of these plasmas. This will require the use of sophisticated, multispacecraft missions, accomplishing direct and remote sensing observations as well as active perturbation experiments. Ground observations and experimentation will continue to provide important long term measurements. Both space-based and ground experimentation will have to be coordinated effectively. Advanced computational techniques will dramatically strengthen theoretical modeling and simulation.

TOOLS FOR SPACE PLASMA PHYSICS

Space-Based Techniques

The mainstay of progress in space plasma physics has been in situ and remote sensing experiments from space. They provide the means for systematically monitoring large regions of space plasma. The inability to distinguish space from time changes in measurements from a single spacecraft underlies the future thrust toward flying constellations of identically instrumented and electronically coordinated satellites to study a given phenomenon or a limited region of space. The success of such efforts will depend strongly on the implementation of self-contained "smart" electronics to facilitate real-time complementarity and software techniques for onboard selection, digestion, and compaction of the plethora of data from multiple sources.

Active experiment techniques are used to create a controlled disturbance and study its effect on the environment. (See Figure 6.1.) Active experiments have a broad range of objectives. These include (1) simulation of natural processes occurring in space plasmas, (2) measurement of physical properties such as reaction rates of atmospheric constituents and collisional cross sections, (3) use of space as a laboratory without walls to study fundamental plasma physics, (4) probing the natural environment as is done in experiments tracing magnetic field lines by electron beams, and (5) improving communication systems by studying the propagation of electromagnetic waves. For the study of space plasma processes the attraction of active techniques is twofold: there is no need to wait for a phenomenon to occur naturally, and the source characteristics are known and can be controlled. In this way, active experiments are similar to laboratory experiments except that the former have the advantage that the space plasma for most purposes can be considered boundless. The disadvantage is that it is difficult to obtain measurements with high spatial resolution. To remedy this problem, multiplatform experiments have become more common in recent years.

Ground-Based Techniques

Although the magnetosphere is an enormous region in space, we benefit largely from the dipolar origin of the field, which causes all the geomagnetic field lines of the magnetosphere to intersect the Earth, and most of them in the polar regions. This focusing of field lines provides a tremendous benefit observationally because arrays of ground-based instrumentation are relatively inexpensive to deploy and operate and they can provide important correlative data as well as a global context within which to interpret satellite data.

Ground-based data also provide a long-term database that permits understanding of the secular variations and changes in the solar-terrestrial "climate."

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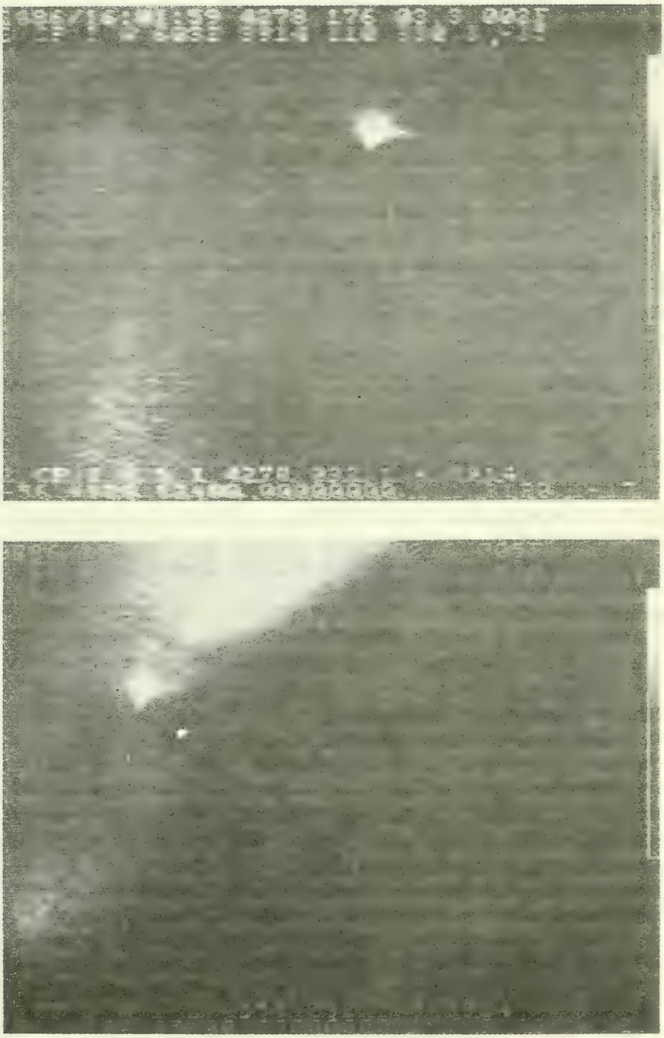

A variety of standard observatories provide continuous measurements of magnetic fields, ionospheric conditions, and solar activity. Also, a number of facilities operate only occasionally, such as the large incoherent-scatter radar facilities.

Plasma Theory and Simulations

Theory must develop a framework for interpreting observations of various physical systems. With this framework as a basis, quantitative analytic and numerical descriptions of model physical systems are constructed and refined through comparison with observations. Ultimately, the model physical systems must be sufficiently similar to the natural physical systems, and the numerical and/or analytic descriptions of these model systems must be sufficiently refined to provide a high level of predictability of the observed behavior of space plasmas. (See Plate 5.)

An outstanding issue involving modeling and simulations is how to properly represent multiscale phenomena numerically. For example, how can one best incorporate anomalous transport processes occurring in boundary layers (such as shocks), which occur on both temporal and spatial scales that are microscopic, into meso- or macroscale numerical codes?

The ultimate test of accomplishment in theoretical and simulation research must be the degree to which closure is achieved with ground and space experiments. Theoretical investigations may be conducted with the goal of explaining physical phenomena that have been observed and measured, and theoretical studies may be designed to lead to predictions testable in space plasma physics missions.

FIGURE 6.1 Example of an active space-plasma experiment designed to study the deposition of energetic electron fluxes in the atmosphere. This controlled experiment, which was flown on the Space Shuttle ATLAS-1 mission in 1992, used an electron beam and an optical imager to study the deposition of high-energy electrons into the polar atmosphere. Shown in the top panel is the artificial aurora generated by the beam (upper right in the figure) and a quiet auroral arc (left). The electron beam pulse was 1 s in duration. The camera viewed downward along the magnetic field direction, and the direction of motion of the Shuttle Orbiter was to the left. The width of the image at a height of 110 km is about 80 km. The bar is linear in optical intensity. The image in the bottom panel is the same as that in the top panel but taken at a later time. It shows the artificial aurora superimposed on a large auroral arc. (Courtesy of S. Mende, Lockheed Research.)

Laboratory Techniques

Historically, laboratory experiments related to space phenomena never have been supported significantly by funding agencies, the exception being specific technology efforts such as electric propulsion. One reason is that it is costly and difficult to build experiments wherein the magnitudes of critical parameters scale with their space counterparts, a necessary condition for the laboratory work to be relevant. However, as a result of advances in general technology (i.e., developments in computers, digitizers, and other hardware), as well as large strides in the design and improvement of plasma devices, it is now possible to perform experiments that were not possible 15 years ago. This increased sophistication allows the possibility of meaningful laboratory simulations. Laboratory experiments can probe a process with unprecedented detail and uncover effects that may not be easily detectable in space. (See Plate 6.)

Laboratory experiments can address both local and global physics issues, the latter often determined by boundaries. In some cases, one can comprehensively analyze physical phenomena simultaneously from both global and local points of view. Furthermore, experimental devices may be rapidly configured to perform new experiments as ideas are developed. This can happen on the time scale of days or weeks. The hardware is reusable and flexible. Many different experiments can be performed on the same machine. In addition to processes directly related to space plasmas, laboratory experiments have provided important technological advances within the areas of spacecraft propulsion and spacecraft potential control. Propulsion devices such as ion thrusters and arcjets, and plasma bridges, such as the plasma contactor, are being tested and studied in space with diagnostics developed largely for ambient plasma observations.

FUNDAMENTAL PROCESSES IN SPACE PLASMAS

Summarized below are a few basic phenomena of wide significance.

Wave-Particle Interactions

The important role of plasma waves in the macroscopic transfer of energy and momentum in space plasmas has become clear as a result of complementary and strongly coupled experimental and theoretical investigations. Over the last few decades, spacecraft observations have provided valuable information regarding the plasma environments around the Earth, planets, Sun, and comets. For example, from a limited set of planetary wave observations, it has become clear that similar waves exist around all the magnetized planets. This would seem to set limits on the significance of anthropogenic effects in triggering natural waves in Earth's magnetosphere. By far the largest body of information on the role of waves has been accumulated in Earth's plasma environment. Spacecraft observations have characterized Earth's magnetosphere as a collection of discrete regions with distinctive physical properties separated by well-defined boundaries. Strong plasma waves have been observed within the boundary regions, particularly the magnetopause and plasma sheet boundary layers. For the magnetopause boundary layer, this has important consequences for the entry of solar wind plasma into Earth's magnetosphere. Spacecraft have mapped out the locale and statistics of occurrence of most important classes of plasma waves in Earth's magnetosphere. Besides being important for boundary region dynamics, plasma waves provide the dominant loss mechanism for energetic electrons in the inner magnetosphere via pitch-angle diffusion and are drivers for the precipitation of ions and electrons into the lower atmospheric regions. Plasma waves are thought to play a principal role in heating ionospheric ions in the topside ionosphere. Upwelling of these heated ions along magnetic field lines and their subsequent trapping in the equatorial plane due to interactions with regions of plasma turbulence provide an important source of magnetospheric plasma. A wide variety of plasma waves participates in the complex energy transfer processes on auroral field lines. Plasma waves have been used as a diagnostic tool to obtain properties of the plasma from both ground-based and space-based systems. Plasma irregularities in the ionosphere occur with scale-size distributions covering tens of kilometers to fractions of a meter. These irregularities, which result from poorly understood instability mechanisms, are a major source for the disruption of high-frequency (HF) and extremely-high-frequency (EHF) communication systems.

Charged-Particle and Plasma Energization

Charged particles in plasma can be accelerated to high energies through a variety of mechanisms, some of which occur in nature or can be induced in suitably arranged space experiments. These include particle acceleration through resonance with quasi-monochromatic waves; stochastic acceleration resulting from resonance overlap due to large wave amplitudes or the presence of a finite spectrum of waves; acceleration by parametric processes, such as beat waves, Brillouin, and Raman scattering; and acceleration by electric fields that result from changes in macroscopic plasma morphology as encountered, for example, on auroral field lines. These phenomena are fundamentally nonlinear and extremely complicated, from both theoretical and observational points of view. Although measurements of electric and magnetic fields can be made with very high time (spectral) resolution, particle measurements are comparatively crude. For many purposes, the particle distribution function must be known to an accuracy that cannot be obtained with present-day technology. As an example, only two of the three velocity components of a distribution are generally known (perpendicular and parallel to Earth's magnetic field). Yet in many resonance interactions with waves, it is the unknown component (the phase coherence) that is the key to the interaction.

Dust-Plasma Interactions

Dusty plasmas are the most common type of plasmas in space. It is now believed that the long-term evolution of the dust and plasma environments is strongly coupled. The dust grains collect electrostatic charges from the plasma, and the evolution of their spatial distribution, size distribution, and lifetime can be determined by electrostatic forces and plasma drag. On the other hand, the dust can alter the plasma composition, density, momentum, and energy distribution, as well as the dispersion relations of the waves propagating in a dusty plasma medium.

In the past decade, a growing effort (laboratory experiments and theory) has focused on problems related to dusty plasmas. We now understand the important processes that determine the charge of the dust grains and have learned the transport processes that shape the fine dust components in planetary rings embedded in magnetospheric plasmas. Magnetospheric perturbations were clearly shown to be responsible for the observed spatial distribution of small dust grains in the Jovian and Saturnian rings. Collective dusty plasma effects were suggested to explain the spokes (transient radial dust features on Saturn's main ring system) observed on Voyager images. The large scattering cross section of charged ice grains in noctilucent clouds is thought to be responsible for the observed anomalous radar echoes. The differential settling of bigger and smaller grains toward the midplane in the early solar system was suggested to cause spatial charge separation that might have resulted in large-scale electrostatic discharges. These lightning bolts could explain the existence of chondrules (small molten beads of rocks found in meteorites).

The Critical Ionization Velocity Effect

Investigations of the critical ionization velocity effect are an important part of space plasma science. The phenomenon involves the nonclassical ionization of energetic neutral atoms and molecules as they move through a background magnetized plasma. From laboratory studies and some space measurements, it is thought that when the center of mass energy of the neutrals rises above their ionization threshold, there is rapid ionization of the neutrals. This process apparently involves energization of the ambient electron gas by plasma waves associated initially with the transformation of a few energetic neutrals to ions. The newly born ions have considerable kinetic energy and heat the electrons through collective plasma processes. When sufficient neutrals are converted to ions, as might happen through charge exchange, for example, the energy density of the electron gas rises to the point where additional ionization of the neutrals ensues, and a flash ionization of most neutrals occurs.

This phenomenon has great significance for models of young solar systems. To perform comprehensive measurements of the processes involved, it will be necessary to achieve the correct physical scale: the electron gas must be heated over a sufficiently large distance that its temperature can rise to the point where impact ionization of the neutrals becomes important to the overall system of interacting gases. Such experiments lie in the future and will require much more extensive supporting resources than have been possible with small free-flying satellites or rockets.

Radiation Processes

The topic of radiation processes is relatively new and involves detailed study of the production, transport, and absorption of microwave, infrared, and shorter-wavelength radiation in dense plasmas. However, its implication to the study of astrophysical systems is profound. The interaction of such radiation with matter involves individual molecules, atoms/ions, or electrons—not collective plasma processes. Clearly, radiation processes are of fundamental importance in transporting energy through portions of the Sun and of the Earth's atmosphere. In addition, radiation propagating freely from its source and from optically thick regions is the primary means by which remote sensing is accomplished. The opportunity to study fully coupled electromagnetic radiation with plasma dynamics in the space environment supplements the extensive work done in laboratory plasmas on similar problems.

ACTIVE EXPERIMENTS

Active experiments have a broad range of objectives. The techniques used in active experiments include four main categories: (1) injection of plasma and neutral vapor; (2) injection of energetic beams of neutral particles, ions, or electrons; (3) wave injection from ground based systems of acoustic waves and electromagnetic waves in the very-low-frequency (VLF) and HF bands, or injection from space vehicles of VLF, HF, and microwave radiation; and (4) use of the spacecraft as a disturbance to study spacecraft wake, vehicle charging, ram glow, or the electromagnetic effects of tethered systems.

Plasma and Neutral Mass Injections

The natural space environment can be modified by the introduction of foreign gases and plasmas to induce or enhance local processes. These include changes of the local ion composition, reduction of the local electron density, changes in the charge state of ions, changes in the average energy of the local plasma, and the plasma's chemical nature. This permits study of various processes occurring within ionospheric plasmas, including production, loss, and transport. It is also a way of creating unstable environments that evolve in interesting and new ways not normally found in the natural environment. The possibility of creating a large-scale ionic plasma (positive and negative ions dominating the overall composition) is both interesting and important in that it allows new processes to become dominant in plasma behavior. Such experiments are difficult, if not impossible, to perform in terrestrial laboratories.

Pulsed plasma beam or contactor experiments, where a dense plasma cloud is released into the ambient medium, can give information on plasma transport. The plasma cloud expands and distorts in response to its internal diamagnetic structure as well as the external flow field. This expansion sheds light on the fundamental plasma physics of high-beta plasma clouds, such as occur in the magnetotail, as well as the nature of the transport process when the cloud is diluted. Such experiments are conceptually similar to those already under way in ground-based laboratories, with an important exception. By using pulses of sufficient density and duration, it is possible to create steady-state diamagnetic plasma regions near the source. Information about the various processes acting in such an unusual plasma configuration is an important step toward understanding a new regime of plasma physics.

Particle Beam Experiments

Particle-beam experiments have been conducted from many sounding rockets, satellites, and space shuttle missions. Objectives in these experiments have been (1) to map the geomagnetic field structure and parallel electric fields by observing echoes of beam electrons from the magnetic conjugate mirror point or from electrostatic structures along auroral field lines; (2) to study auroral processes, such as optical emissions and wave turbulence in auroral particle beams; (3) to stimulate electromagnetic and electrostatic wave excitation; (4) to create suprathermal electron tails; (5) to observe the interaction of the particle beam with the neutral gas in the vicinity of the source payload; and (6) to study spacecraft charging and neutralization.

Wave Injection Experiments

Space-based wave injection experiments make use of a number of different techniques to launch waves into the plasma. Topside sounders rely on the excitation of plasma resonances. Transmitters have been used on a number of sounding rockets. Finally, modulated electron beams have been used as "virtual antennas" on STS-1 and the Spacelab-2 shuttle missions and on numerous sounding rocket experiments. Waves have been detected to a distance of a few kilometers. In these experiments, receivers have been located on the transmitter platform, on a subsatellite, or on the ground. Objectives include the study of antenna properties, near-zone electromagnetic field studies, wave propagation, and wave-particle interactions.

Ground-based HF wave (megahertz-range) injections are launched from powerful radars into the ionosphere under quiet conditions, during magnetic storms, or in conjunction with chemical releases from active experiments. A large number of plasma processes can be studied in this way: focusing or defocusing of the beam, interference with communications, heating of the plasma, generation of suprathermal electron fluxes and airglow, instabilities including self-focusing, parametric interactions and strong Langmuir turbulence, generation of plasma irregularities, focusing by chemical releases, and effects of a hierarchy of heater thresholds. In addition, pulsed HF heating is used to modulate the auroral electrojet current for extremely low frequency (ELF) or VLF wave generation. VLF wave experiments study wave-induced particle precipitation, earth-ionosphere wave guide modification, stimulation of VLF emissions, direct D-region heating, and ELF-modulated VLF to produce a polar electrojet antenna. Strong acoustic waves from explosions generate gravity waves and acoustic shock waves that couple into the plasma through collisional interaction.

Vehicle-Environment Interactions

A space vehicle perturbs the environment in a number of ways. Out-gas clouds, fluid dumps, and thruster firings interact with the ambient plasma much as the neutral gas injections described above. In addition, the structure itself creates a wake in the plasma, in particular for orbiting platforms, for which the spacecraft velocity generally is larger than the ion thermal velocity. Surface glow induced by neutral atmosphere interactions with the spacecraft surface has been studied in the case of the space shuttle. High-voltage power systems and their interaction with the ionosphere were studied in sounding rocket experiments. Objectives were to study the plasma sheath, the charging levels, and the steady-state currents in the ambient plasma.

Processes associated with the physical contact between plasmas and exposed surfaces in space are an important practical aspect of many advanced scientific and technological space systems. For example, the ability to draw electron current from magnetized space plasmas is an essential feature of plans for power-producing electrodynamic tether systems. Charging of dielectrics in the vicinity of high-current beam experiments is similarly an important concern. It is striking to realize that while basic issues of plasma sheaths and current extraction have been known for more than 50 years, we still lack fundamental knowledge of the processes involved, especially at high voltages and currents. Relatively simple experiments, such as measuring the voltage-current collection curves for magnetized plasma, have yet to be done for ranges of parameters in which large-amplitude plasma waves play an important role.

Much of our present knowledge of plasma sheaths comes from laboratory measurements. In the case of electron current collection, this has imposed severe limitation on the scale of phenomena that can be studied. Because the total number of electrons in a given plasma chamber is limited, laboratory measurements of electron current are limited in time and current density to very small values. Measurements in space offer a far better situation since in space one can place the collecting anode in an essentially unbounded medium.

FUTURE PLANS AND OPPORTUNITIES

In Situ Observations

The major task ahead in our studies of naturally occurring space plasmas is to obtain the information necessary to understand and elucidate processes that control their physical behavior. This will require the use of sophisticated, multispacecraft missions containing the latest technology in direct and remote sensing instrumentation. The technology to carry out these studies exists today. We now know how to construct rugged, reliable, and lightweight instruments capable of making three-dimensional, high spatial and temporal measurements of particle fluxes. We have demonstrated that we can make detailed measurements of electric and magnetic wave phenomena and have had great success in making remote optical measurements from UV to microwave frequencies.

There are plans to make some of these important measurements in the decade ahead, using relatively small, as well as larger, spacecraft programs. Some focused studies can be carried out with a single low-cost spacecraft. An example of such a mission is the Fast Auroral Snapshot (FAST) Explorer. It is a relatively low-cost mission planned to be launched in 1995. Its aim is to study the plasma microphysics of the terrestrial auroral zone, and it will make very-high-resolution measurements triggered by certain preprogrammed signatures.

The possibility of even cheaper, but still highly sophisticated missions, using leading-edge civilian and military dual-use technology, is currently under study. The microelectronics revolution has enabled the design of small, fast, smart, less expensive instruments, compared with the standards of a decade ago. Microprocessors, use of higher-order software languages such as C++, and specialized semiconductor chips, which enable digital signal processing, analog-to-digital signal conversion, and other operations, can be built into instruments, providing wide flexibility and speeding up changes in operating modes and other functions. Specialized analog systems under digital control permit rapid and accurate changes in voltages, currents, and other important aspects of instrument operation. As a consequence, aperture sizes have shrunk toward theoretical limits, detector systems have become miniaturized, detector efficiencies have become high, power consumption has become very low, and data rates are fast enough to challenge every satellite or suborbital rocket system designer.

Furthermore, the development of microprocessor systems capable of controlling all aspects of remote experiments has opened the way to new concepts of experiments, enabled by high data bandwidths and precise timing of the process or events under consideration. The combination of rapid switching of instrument modes, linked to high bandwidth data acquisition, and the ability to analyze data onboard the instrument platform and alter the course of the data taking, is a feature that is not fully in place but that opens the way to much more precise observations of plasma phenomena in space.

However, many, if not most, of the major outstanding questions in space plasma physics require sophisticated and coordinated multiple satellite missions to provide the much needed ability to distinguish between spatial and temporal changes. This is not a new concept. The Global Geospace Science (GGS) Program is a part of the International Solar-Terrestrial Physics Program (ISTP) and consists of three satellites—Geotail, launched in 1992, Wind, launched in 1994, and Polar, to be launched in 1995. The planned separation distance of these satellites is very large; thus, their mission is to study long-range correlations. Cluster, a European Space Agency (ESA) program, planned to be launched in 1995, is the first constellation mission. Four essentially identically instrumented satellites are planned to fly in a tetrahedral formation, with variable separation, which at times will be as small as a few ion Larmor radii. Another mission in the study phase, the Grand Tour Cluster (GTC), is aimed at studying low-latitude magnetospheric structures even smaller than an ion Larmor radius.

All recent large-scale programs, such as GGS and Cluster, have involved international cooperation. Such joint endeavors are both necessary and desirable. Involvement of international partners not only decreases the cost involved for the participating nations, but also ensures that the best available technology is used and the best scientists are participating, thus enhancing the scientific return. Careful coordination of ground-based observations with satellite-based measurements also will lead to significant increases in the scientific return from such programs.

Solar physics in general, and solar plasma physics in particular, are different in the sense that our knowledge to the present has been obtained largely from Earth or Earth orbit. The perspective—but not the proximity—changed in the fall of 1994 with the passage of the Ulysses spacecraft high above the Sun's south pole at a radial distance of about 2 au (where 1 au is the Sun-Earth distance). Despite the resultant limitations of poor spatial resolution, we infer strongly that a rich variety of plasma phenomena are occurring and that plasma physics is complementary to nuclear physics in determining solar structure and behavior. The Sun is a source of magnetic field, which probably is generated by the interaction between its differential rotation and MHD convection in its interior. The body of the Sun supports a plethora of waves, which manifest themselves through surface oscillations, whose study has given rise to the field of helioseismology. The magnetic field reaching the surface, rather than being uniformly distributed, is concentrated in small flux tubes, many having loop topology, so that the solar corona, as viewed in x-rays (see Plate 7), is a veritable archive of plasma structures: sunspots, fibrils, prominences, spicules, holes, bright spots, and so on. The corona is a dynamic region, the source of both a continuous solar wind and, from time to time, blobs of localized, energetic plasma flow that drive shock waves ahead of them as they propagate outward toward the planets. Such coronal mass ejections (CMEs) usually cause ground-based electromagnetic disturbances when they hit the terrestrial magnetosphere. Solar flares are associated with many CMEs and are one of nature's most observable examples of particle acceleration. Radiation spanning the electromagnetic spectrum is generated either directly by wave-particle processes or secondarily by energetic particles interacting with chromospheric material. In addition, flare-related relativistic electron beams propagating into the solar wind produce there characteristic (Type III) radio waves by processes that are generally thought to be highly nonlinear in nature.

Understanding of plasma activity on the Sun would undoubtedly prosper from a high-resolution, FAST-type mission. That is impossible because of the distant and more hostile environment. The closest approximation is the Solar Probe spacecraft, currently under study, which would make a one-time pass within three to four radii of the nominal surface. Besides providing scientific insight through in situ observations, Solar Probe presents obvious technical challenges in the area of thermal engineering.

In the meantime, the ESA Solar Optical and Heliospheric Observatory (SOHO) is being prepared for a 1995 launch, with U.S. participation in the instrument complement. The scientific objectives of SOHO are to study localized plasma structures—loops, prominences, holes, flares, mass ejections, and so on—in the solar chromosphere, transition region, and corona by spectroscopy and imagery of their electromagnetic emissions at UV and visible wavelengths and, at the same time, to monitor derivative solar wind effects via onboard particle measurements. Additionally, data from instruments that measure fluctuations in solar brightness and coherent, long-wavelength oscillations of the solar disk, so called helioseismology, may shed light on processes occurring in the solar interior.

Cassini, another mission in an advanced state of development, is currently being developed as a mission to Saturn scheduled for launch in 1997. Using a new technique, one instrument will be able to form a two-dimensional image, providing the direction of arrival of energetic neutral atoms formed via charge exchange with energetic ions. The results will enable the measurement of the spatial extent and energy composition of the large plasma zones surrounding Saturn and its satellites.

Much future work in planetary science will focus on waves and instabilities in naturally occurring dusty plasmas. The Ulysses, Galileo, and Cassini missions will fuel more interest in this field. Data from dust detectors, imaging, plasma and plasma wave experiments, magnetic field measurements, and so on will be used to understand dusty plasmas in planetary magnetospheres and in the interplanetary medium.

An important new mission to study the ionized and neutral upper atmosphere of Earth is slated for a new start within the next year. The Thermosphere-Ionosphere-Mesosphere Dynamics (TIMED) mission will carry a variety of instruments designed to probe complex interactions affecting the behavior of Earth's atmospheric regions lying above the stratosphere. The upper atmosphere has a factor of 10 greater response to global warming than the lower atmosphere, and can serve as an indicator of subtle changes that may be either anthropogenic or externally driven. TIMED will study this altitude regime, which experiences coupling between neutral and plasma constituents, and where competition between solar irradiance variation and plasma processes such as joule heating is important. Plans for deployment of an Earth Observing System as part of NASA's Mission to Planet Earth should incorporate as a component of the program the study of plasma coupling to the neutral atmosphere.

A major difficulty with space satellite constellation experiments is that differences in satellite altitudes lead to different orbital periods. Coordinated local observations thus become a matter of occasional opportunity, and a concentration of observations from different satellites at one time will rapidly decay to widely dispersed observations over times of a few minutes. NASA is developing a new way to obtain coordinated measurements over distances up to several hundred kilometers. This involves the use of long tethers connecting individual satellite platforms together. In a static configuration, the instrument string (looking much like a deep-sea string of acoustical sensors) is deployed along a vertical direction. The entire system moves with a constant, common angular velocity with respect to the Earth. This results in the possibility of obtaining simultaneous plasma and atmospheric data over a wide range of altitudes. Such a system could be used to observe the high-altitude acceleration zone for auroral electrons, the possible presence of horizontal plasma shear in large-scale plasma convection in the polar caps, or the behavior of aurora plasma in the regions of atmospheric excitation.

A substantial number of other missions exploring the behavior of space plasmas are now being planned by the U.S. and international scientific communities, with the Solar-Terrestrial Energy Program (STEP) providing coordination of both ground- and space-based systems. As in the past, there is a strong sense of cooperation among the international participants. An extensive and specific evaluation of future space missions, to be entitled *A Science Strategy for Space Physics*, is currently in progress under the auspices of the NRC's Committee on Solar and Space Physics and Committee on Solar-Terrestrial Research. When completed, this study will be used by NASA in its planning of new missions over the coming decade.

In Situ Experiments

The previous section concentrated on passive observations of natural processes acting in space. It is also possible to conduct in situ, or active, experiments whereby artificial injections of charged particles, neutral gases, or electromagnetic waves are used to alter natural processes or to stimulate new processes in the ambient plasma medium. It is possible to contemplate a rich selection of potential in situ experiments capable of exploring new areas of plasmas in space. NASA is the principal sponsor of such work, but for the past four years, as a matter of policy, NASA has restricted its funding to those projects that explore natural processes, rather than artificially induced behaviors. However, since many of the results of the latter types of experiments have important implications for plasmas in different space environments, it is hoped that this policy will be reviewed. Here we give brief outlines of some possible in situ experiments that have special merit.

Space vehicles offer the promise of performing three-dimensional experiments in unbounded plasmas with varying mixtures of neutral gas. These can be done on a scale size that should make the instrumentation easy to build. In addition, the relevant time scales are microseconds or longer, which are easily measured and recorded. In spite of these advantages, plasma experiments in space have not been easy to perform. The principal reasons are that diagnostic instruments are difficult to place accurately and the space platforms that carry them may be big enough to interfere with the experiment.

By using space platforms with suitable resources, it should be possible to investigate steady-state diamagnetic cavities in space plasmas. In this situation, the plasma effusion speed from its source can be made larger than the diffusion speed of the magnetic field. A complex region of low magnetic field is maintained by plasma pressure against the flowing ambient plasma and ambient magnetic field. This is an unstable situation, which opens the way to investigation of various types of instabilities. It is likely that these will reveal the presence of many new high-beta plasma-magnetic field interactions that depend on various plasma and magnetic field parameters. Magnetic field interactions, analogous to the solar wind-geomagnetic field coupling, can also be anticipated as the capability to construct and operate large magnets in space evolves. These experiments, involving a variety of plasmas and magnetic field configurations, will have relevance to a wide range of astrophysical situations.

Terrestrial Observation Networks

Support for the existing standard observatories, which provide the long-term monitoring of fundamental parameters of the upper atmosphere, ionosphere, and magnetosphere, is a key part of a scientific strategy that recognizes the importance of time series data relating to the geophysical environment. Optical, radar, geomagnetic, and other instruments provide an important view of external plasma processes, and data acquired simultaneously from many sites provide a basis for understanding many different manifestations of magnetospheric and ionospheric dynamics that are closely linked to the solar wind. Recommendations from the scientific community include increasing the number of operating stations, as well as modernizing them, to enable development of precise and high-quality databases.

Lack of observations from the Southern Hemisphere, particularly digital data, is a serious problem. These data are necessary to understand the asymmetries that arise from summer-winter differences in the polar ionospheres and from asymmetries in the geomagnetic field itself. Digital data acquisition in the Antarctic is particularly important, and the Antarctic is the only region where stable instrument platforms can be easily placed at very high polar latitudes (in the polar cap).

Equal emphasis must be given to global arrays and to dense regional arrays of instruments. Concern should be paid to including complementary instruments within arrays in order to achieve a rich source of fundamental parameters. Arrays must also be utilized to deconvolve the spatial and temporal aliasing of the data. This is a particular problem with the interpretation of data from a single station or spacecraft. Thus, the coordinated use of multiple stations and multiple instruments should become increasingly the norm in data analysis.

Laboratory Experiments

When a phenomenon has been identified by a spacecraft and the basic physics of it is not well understood, the laboratory is the ideal place to study it. The problems encountered in space observations of single-point measurements and nonrepeatability are overcome in the lab. A well-planned experiment can be carefully tailored so that it is repetitive in space and time. Plasma laboratory technology has advanced to the point that many experiments pertinent to space plasma phenomena can be performed. For example, in wave studies, waves can be made linear or nonlinear by the turn of an amplifier knob. Furthermore, these waves can be launched from one or more antennas and their fields mapped in the near and far zone. Beams can be introduced from localized sources, density nonuniformities can be repeatably produced, impurities can be added in known amounts at a given location, and plasma drifts can be created. Furthermore, measurements can be acquired at thousands of three-dimensional spatial positions and thousands of time steps during the interaction. This is impossible in space. Laboratory experiments can address both local and global physics issues (the latter are often determined by boundaries). In some cases, one can comprehensively analyze physical phenomena simultaneously from both global and local points of view. Furthermore, experimental devices may be rapidly configured to perform new experiments as ideas are developed. This can happen on the time scale of days or weeks, as contrasted with many years for satellites and several years for rockets. The hardware is reusable and flexible. Many different experiments can be performed on the same machine.

The challenge to laboratory plasma science is to continue to develop technology in order to extend the range of physical phenomena that can be studied. It is now possible to fabricate microscopic detectors and antennae that are capable of making spatially resolved, in situ measurements of the electric and magnetic fields in the plasma, the electron and ion temperatures, the plasma potential, and the velocity distribution functions. Nonperturbing optical techniques, such as laser-induced fluorescence and optical tagging, are now well established. Other new techniques are time-resolved tomography, electron cyclotron emission spectroscopy, and the use of the motional Stark effect. Three-dimensional probe systems can move detectors (optical or electronic) anywhere within large devices, so that full space-time data sets can be acquired. Visualization software and three-dimensional-graphics computers make analyzing these data possible.

Scientific areas in which laboratory simulation experiments can be carried out with current technology include properties of Alfvén waves, magnetic field line reconnection, wave-particle interactions leading to chaos, and current modulation of plasma conductivity.

CONCLUSIONS AND RECOMMENDATIONS

Space plasma physics, as the study of natural plasmas and associated technological applications, represents a vast multiscale physical domain with large variations in plasma sources, average thermal energy, flow velocities, magnetic field strength, and other underlying physical processes. As such, it represents an important regime for plasma science and technology and for our civilization. First, it provides us with an understanding in quantitative terms of the variety of interrelated complex processes acting to shape and influence our own terrestrial environment. Second, it affords the opportunity to observe at closer hand phenomena that may be operative in astrophysical situations. Third, space phenomena stimulate fundamental scientific questions relating to the behavior of plasmas under conditions that can be very different from those created and studied in terrestrial laboratories. And finally, space plasma science underlies the development of technological applications operating in or based on the space plasma environment. As a consequence, investigations of natural space plasma processes extend the frontiers of human knowledge, enabling broader physical understanding of plasmas within the context of their general behavior.

Progress to date in understanding the space plasma environment has provided us with a broad picture along with some detail. However, many important details of physical mechanisms remain unanswered, including interdependencies between sources and physical responses. Successful investigation of this environment requires a coordinated and balanced approach utilizing in situ observations, active experimentation, theoretical modeling, ground observations, and laboratory simulations. The requirement for a high degree of synergism is an inescapable conclusion.

The cornerstones of space plasma physics are observations carried out, analyzed, and interpreted in conjunction with complementary theory and modeling. Space plasma physics has historically developed in this mode. With the advent of new technologies, opportunities for further scientific understandings are nearly limitless. In studies similar to this one, the space community is currently examining future directions and independently identifying possibilities. The panel supports such efforts.

The use of space as a medium for active experimentation has declined to the point of near extinction. This is unfortunate, since active experiments may elucidate natural processes and expand in a unique way our basic understanding of the plasma state. The panel recommends a reinvigoration of the active experimentation area.

Meaningful laboratory experiments simulating space phenomena can now be performed in a number of different problem areas. Such experiments provide the opportunity to examine the relevant science in a controllable and reproducible manner; they are thus an important adjunct to highly transitory space observations and can hence serve as a vehicle for interpreting, substantiating, and/or planning the latter. Such laboratory experiments have been largely discredited in the past because they did not scale properly to space conditions, but that shortcoming has been circumvented by developments in technology. The panel recommends an initiative in laboratory experiments of sufficient magnitude to establish a small interactive community.

7

Plasma Astrophysics

Plasma physics is relevant to almost every area of astrophysics, from magnetized, highly conducting stellar and interstellar plasma to gravitationally interacting many-body systems such as star clusters and galaxies. In some cases, the plasma physics is quite standard and requires only the application of known results. In other cases, the problem lies beyond the current frontiers of knowledge. Yet, plasma physics is not part of the standard graduate astrophysics curriculum, and plasma astrophysics has no distinct home at any federal funding agency. This chapter briefly describes some recent accomplishments and outstanding problems in plasma astrophysics, as well as education in and funding of plasma astrophysics.

RECENT ACCOMPLISHMENTS IN PLASMA ASTROPHYSICS

Any list of recent accomplishments is bound to be incomplete, but the work discussed below is representative.

Magnetized Disks, Winds, and Jets

Astrophysical interest in this problem goes back at least as far as the 1950s, when Fred Hoyle speculated that the early Sun could have transferred angular momentum to the protoplanetary disk via magnetic torques. The first quantitative theories began with the solar wind, which is observed to be magnetized. Simple models were developed to show that magnetic torques exerted by the solar wind could have removed significant quantities of angular momentum from the Sun and, by implication, that magnetized winds could play an important role in spinning down stars. Later, spurred by observations of accretion disks and jets around a wide variety of objects including protostars, white dwarfs, neutron stars, and black holes, astrophysicists developed models of magnetized winds and jets in disk geometry, included relativistic effects, strong magnetic fields, rapid rotation, and the effects of MHD waves and instabilities on the disks and the outflows. (See Figure 7.1.)

Particle Acceleration in Shocks

Although our understanding of high-Mach-number shocks is seriously incomplete, studies of particle acceleration in shocks have given us the best theories to date of cosmic-ray acceleration in the interstellar medium. The most notable successes of the theory are that it predicts approximately the correct power-law index of the energy spectrum, cosmic-ray intensity, and cosmic-ray composition (with the exception of the electron-to-ion ratio). Progress has been made on the analytical front through both kinetic and hydrodynamical descriptions of the particles and the shock and on the computational front through Monte Carlo simulations.

Magnetized Convection in Stars

The subject of stellar convection has a long history, since it was recognized many years ago that the radiative energy flux through a stellar envelope is limited by convective instability. Interest in the interaction of magnetic fields with convection stems from observations of the solar magnetic activity cycle and similar cycles on other stars, which show that magnetic fields are rapidly regenerated and reconfigured in the interiors of convective stars. Until recently, stellar convection was described only by dimensional arguments or mixing length theory. With the development of parallel and massively parallel computer architecture, it has become possible to simulate compressible convection in three dimensions and to include the effects of magnetic fields. Although the smallest relevant length scales are still unresolved by these calculations, the effects of buoyancy, concentration of flux into ropes, and dynamo activity—all processes that are believed to play an important role in the dynamics of stellar magnetic fields—are observed and can be studied.

Formation of Low-Mass Stars

It was recognized long ago that the ratio of magnetic flux to mass is much higher in the interstellar medium than it is in stars. It was proposed that interstellar clouds are supported against their gravitational fields by magnetic forces, that the fields slowly escape from the clouds by ion-neutral relative drift, and that the

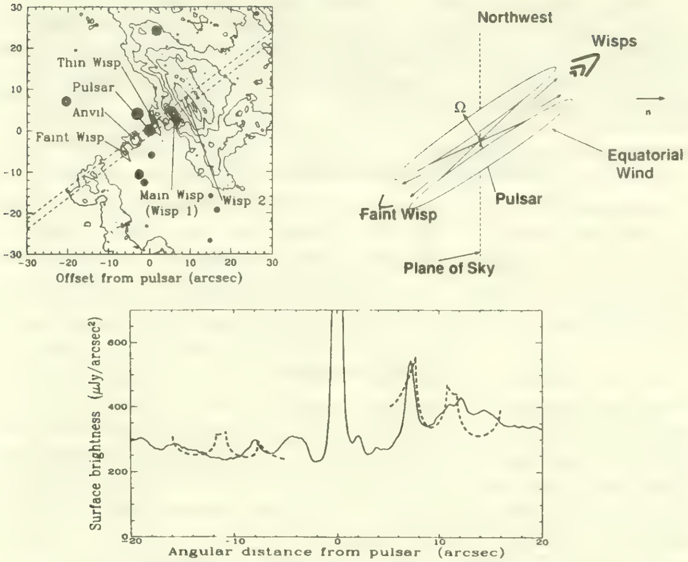

FIGURE 7.1 A plasma kinetic-theory model of a relativistic shock wave in the Crab Nebula. Upper left: Contour plot of the surface brightness of x-ray emission at 0.8 Å. The "wisp" features are thought to be visible manifestations of the otherwise radiationless outflow of rotational energy from the central pulsar. Upper right: Geometry of the outflow from the pulsar used in the construction of the theoretical model. The pulsar is assumed to lose energy in the form of a magnetohydrodynamic wind, flowing relativistically in an angular sector around the rotational equator of the pulsar. The magnetic field direction is orthogonal to the radial flow. The wind's composition is a mixture of electrons, positrons, and heavy ions, and it is quasi-neutral in the region upstream of the shock wave that terminates the outflow. Estimates indicate that a shock wave forms in the region of the observed wisps. The vector **n** points toward the observer. Lower panel: Comparison of the surface brightness (solid line) measured in the strip between the dashed lines in the upper panel with that predicted by the model (dashed line). The model represents the electron-positron pairs as a relativistically hot Maxwellian fluid, heated by the collisionless subshock at the leading edge of the shock structure. Heavy ions are modeled as a stream of particles gyrating in the electromagnetic field of the shock, compressing the magnetic field and pair plasma at each turning point of the ions' orbit. Each such compression appears as a surface brightness enhancement. The model successfully predicts the brightness of the faint wisp at −7 arc sec. (Reprinted, by permission, from M. Hoshino, J. Arons, Y.A. Gallant, and A.B. Langdon, Astrophysical Journal 390:454, 1992, and Y.A. Gallant and J. Arons, Astrophysical Journal 435:230, 1994. Copyright © 1992, 1994 by the American Astronomical Society.)

clouds collapse and form stars once a sufficient amount of their magnetic flux has been removed. This picture has been confirmed and extended by extensive theoretical calculations, including static models of magnetized, self-gravitating clouds; dynamical models of gravitational collapse; and both analytical and numerical calculations of ion-neutral drift.

PROBLEMS IN PLASMA ASTROPHYSICS

The panel has not attempted to make an exhaustive list of problems in plasma astrophysics, but instead has chosen a few problems that arise in a broad variety of physical environments, illustrating how plasma physics touches almost every part of astrophysics. Some of these problems are in the realm of space plasma physics as well.

Dense Stellar Plasmas

The mass density and temperature at the center of the Sun, which is an ordinary, low-mass star, are predicted to be about 100 g/cm^3 and 2×10^7 K, respectively. The energy produced by nuclear reactions diffuses outward as radiative energy, with most of the opacity due to bound-free transitions in elements heavier than helium. At these temperatures and densities, atoms are significantly perturbed by their nearest neighbors. Recent attempts to take these many-body effects into account when calculating the opacity and equation of state of dense stellar material have produced strikingly different results from earlier calculations, which has injected substantial uncertainty into models of solar-type stars and their evolution. This problem is at the intersection of plasma physics, statistical mechanics, and atomic physics.

Thermal Conduction in Plasmas

Observations suggest sharp temperature interfaces between the solar corona and lower atmosphere and at the boundaries of interstellar clouds. These interfaces are sharp in the sense that the inferred temperature scale height is comparable to the electron mean free path. The transport of heat becomes strongly nonlocal, and the electron distribution function becomes non-Maxwellian. Attempts to solve this problem have ranged from the application of theories of saturated heat flux regulated by ion-acoustic instabilities to attempts at full kinetic theory solutions of the Boltzmann equation.

Structure of Collisionless Shocks

Astrophysical shock waves are produced by energetic, impulsive events ranging from solar and stellar flares to sequential supernova explosions in associations of massive stars. Because mean free paths are long, these shocks must be collisionless. Remote sensing by spectroscopy shows that electrons as well as ions are heated to high temperatures. How is the ion distribution thermalized? How is this energy fed into the electrons? How is a small tail of particles accelerated to high energies, as is observed in the interplanetary medium? What is the back-reaction of the accelerated particles on the shock? These remain outstanding problems, because the Mach numbers are so high that the shocks are probably turbulent. Numerical simulations appear to be the most promising way to attack the problem at this point.

Acceleration of Particles to High Energies

Spiral galaxies appear to be permeated by a component of energetic particles, cosmic rays. In our galaxy the distribution function can be followed from subrelativistic energies to energies as high as 10^{21} eV. The most energetic particles cannot be confined by the galactic magnetic field. The energy density of these cosmic rays is similar to both the magnetic and the turbulent energy density in the galactic disk. How are these particles accelerated, and how do they propagate through the galaxy? The prevailing theories have particles at energies less than about 10^{15} eV accelerated by the Fermi mechanism in shocks and predict that they will be trapped within the galaxy by resonant scattering off Alfvén waves excited by their own anisotropy. For more energetic particles, the confinement is problematic, and the origin may be extragalactic.

Hydromagnetic Turbulence

There is abundant evidence for hydromagnetic turbulence in objects as diverse as stellar convection zones, the interstellar gas in galaxies, and the gas in clusters of galaxies. Turbulence can provide hydrodynamic forces (e.g., pressure support in interstellar clouds or acceleration in stellar winds), can lead to transport coefficients such as viscosity or resistivity that are much larger than their molecular values, and can provide significant heating through dissipation. Yet, we do not have a complete theory of hydromagnetic turbulence, and simulations, which are of great educational value, do not yet resolve the full range of relevant scales. Progress in understanding MHD turbulence will probably be made through a combination of direct observation (such as in situ measurements in the solar wind), simulations, and analytical theory.

Magnetic Reconnection

The magnetic Reynolds number (or Lundquist number) of astrophysical plasmas is typically huge, ranging from 10^8 in the solar interior to 10^{21} in the galactic interstellar medium. The naive conclusion is then that magnetic flux is permanently frozen into the plasma and that the field never changes topology. Yet, magnetic fields apparently do change topology (e.g., the star formation process seems to reconnect the magnetic field), and there is strong evidence that magnetic reconnection is an important source of energy in solar flares. How do field lines reconnect at very high magnetic Reynolds number? Present thinking suggests a two-stage process: some ideal magnetohydrodynamic effect creates steep gradients; then reconnection proceeds. We need a more fundamental understanding of the reconnection process itself; many of the fusion-oriented simulations have inappropriate boundary conditions for astrophysical systems. We also need a better understanding of the "ideal" current concentration phase.

The Magnetization of the Universe

Stars, galaxies, and the gas in clusters of galaxies possess magnetic fields. Standard cosmology predicts that the big bang did not produce a magnetic field. How and when did the universe become magnetized? Did large-scale, intergalactic fields form first and become incorporated into smaller structures, or did fields form first in stars, which then seeded their ambient medium through winds and supernova explosions? Are astrophysical magnetic fields nearly permanent, as suggested by their very long ohmic decay times, or are they constantly destroyed, regenerated, and reconfigured by turbulent dynamos?

Laboratory Experiments

There have been few laboratory experiments dedicated to plasma astrophysics, and any such experiments must carefully scale properly from the laboratory to the real astrophysical system. Areas in which experiments could be helpful include MHD turbulence, magnetic reconnection, shock waves, particle acceleration, dusty plasmas, and heat conduction. The status and future promise of laboratory experiments in many of these and related areas are discussed in Chapter 8.

TRAINING IN PLASMA ASTROPHYSICS

How do graduate students become equipped to deal with problems in plasma astrophysics? The standard graduate curriculum in astrophysics contains graduate physics courses, such as quantum mechanics, electrodynamics, statistical mechanics, classical mechanics—more or fewer, depending on the school and the inclination of the student. Then there are standard astrophysics courses, such as stellar structure and evolution, stellar atmospheres and radiative transfer, interstellar medium, and galaxies and cosmology. At many universities, no courses in plasma physics are taught in the physics or astrophysics departments. Such courses may be given in an engineering or applied science department, but these often have too technological an orientation to attract astrophysics students. Some plasma physics may or may not be integrated into one or more of the astrophysics courses, depending on the inclination of the instructor. Therefore, very few astrophysics students receive much formal exposure to plasma physics, and many astrophysicists view it as an arcane specialty.

Many astrophysicists would like to learn more plasma physics when motivated to do so by developments in their subject. For example, recent measurements of magnetic field strengths in dense, star-forming interstellar clouds have shown that the fields are large enough to strongly affect or even dominate the dynamics. This has spawned a real interest in MHD among interstellar medium researchers, and a number of people who ignored magnetic fields throughout most of their careers are now writing papers on them.

Such people would benefit from a good, modern text on plasma physics, one not oriented toward fusion plasmas, but stressing astrophysically interesting applications and using astrophysically relevant parameters and boundary conditions. Such a book could consist of chapters contributed by experts, provided that a good editor and refereeing system kept the quality high. Such a book could also be used for a graduate course or seminar.

FUNDING FOR PLASMA ASTROPHYSICS

Most plasma astrophysics by individual investigators is funded through the NSF and NASA. Some solar and space plasma physics has been funded by the Air Force and the Office of Naval Research, and some DOE funding has arrived through support for national centers.

Plasma astrophysics funding at the NSF suffers from a problem common to all of theoretical astrophysics: programs are organized by wavelength band or class or object, rather than by physical process. This discourages broad proposals. Yet, one of the exciting aspects of plasma astrophysics is that the same processes are at work under many different astrophysical conditions. Both the 1980 and 1990 NAS-sponsored decadal surveys of astrophysics (the Field[1] and Bahcall[2] Committees, respectively) recommended that the NSF establish a theoretical astrophysics program.

With the notable and important exceptions of its Astrophysical Theory and Space Physics Theory programs, NASA tends to support research focused on its missions. This has led to better support for space plasma physics, where much of the data is mission-relevant, than it has for plasma astrophysics, where funding to develop new theories of distant objects is sometimes deemed an unacceptable stretch of the NASA mission. The panel would welcome an expansion of funding for basic research at NASA.

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[1] National Research Council, Astronomy Survey Committee, *Astronomy and Astrophysics for the 1980s*, National Academy Press, Washington, D.C., 1982.

[2] National Research Council, Astronomy and Astrophysics Survey Committee, *The Decade of Discovery in Astronomy and Astrophysics*, National Academy Press, Washington, D.C., 1991.

SUMMARY

Plasma astrophysics is an exciting and important area of astrophysics that is relevant to a wide variety of astrophysical phenomena and draws on an equally wide variety of topics in plasma physics. Problems in plasma astrophysics could stimulate important basic research. Yet the potential of plasma astrophysics is underrealized. The field is small and lacks the critical mass to provide graduate training at most institutions, especially given the lack of a suitable textbook. The funding base is diffuse, and plasma astrophysics is not recognized as a branch of astrophysics at any federal funding agency. Both the educational and the funding aspects need to be addressed to bring plasma astrophysics into the mainstream.

CONCLUSIONS AND RECOMMENDATIONS

Conclusions

Plasma astrophysics deals with phenomena and problems that are important to virtually every branch of astronomy and astrophysics. Some of these problems touch on areas that are central to basic plasma physics and have indeed inspired research in basic plasma physics. Yet, plasma astrophysics is not recognized as a coherent discipline by any federal funding agency.

Recommendation

The panel recommends that interdisciplinary programs be established at the National Aeronautics and Space Administration and the National Science Foundation with the goal of funding research in plasma astrophysics, whether through astronomy, solar-terrestrial research, physics, or computer sciences. The purpose of such programs would be to encourage research in plasma astrophysics, including research on basic processes that are relevant to many astrophysical systems.

PART III

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Broad Areas of Plasma Science

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Basic Plasma Experiments

INTRODUCTION AND BACKGROUND

Plasma physics deals with the behavior of many-body systems under the influence of long-range Coulombic forces. Plasmas are inherently nonlinear media and, in the presence of a magnetic field, they are also anisotropic. Consequently, plasmas are capable of sustaining a wide variety of waves and instabilities. Plasmas can support three-dimensional currents and exhibit nonlocal behavior and "memory effects" (e.g., within the particle distribution functions). Plasma instabilities can lead to chaotic particle motions, to intricate wave dynamics, and to turbulence. Thus, understanding plasma phenomena involves fundamental aspects of statistical mechanics, fluid dynamics, electrodynamics, and frequently, atomic physics.

Progress in basic science has historically relied on a close interaction between experiment and theory. This is particularly true of plasma physics, where nonlinear and nonequilibrium phenomena in many-body systems are of central importance. In striking contrast to the central importance of laboratory experiments to this field, it is the finding of the panel that activity in and support for basic experiments has decreased markedly over the last two decades. For example, at the 1973 plasma physics division meeting of the American Physical Society, there were 126 papers on basic experimental plasma physics. In contrast, at the 1992 meeting, there was no general session on basic laboratory experiments, and there was only one poster session on laboratory experiments related to space plasmas. At this meeting, there were only about 30 experimental papers on basic plasma physics that were not related to a particular application, and half of these papers were on nonneutral plasmas. It is the conclusion of the panel that the level of activity in basic plasma experiment in the past 20 years almost certainly has been lower than it would have been if there had been in place a well-planned and balanced program in basic plasma science in the United States. The danger is that basic experimental plasma science will disappear in this country, unless one or more funding agencies assume the responsibility to support a critical mass of scientists in this area.

A survey of the plasma science community in the United States, conducted by the panel, shows that renewed support for basic laboratory plasma experiments is its highest priority. The panel has come to this same conclusion: The highest priority in establishing a healthy plasma science in the United States is renewed support for basic experimental research in plasma science. This conclusion coincides with the principal findings of the Brinkman report, *Physics Through the 1990s:*[1]

> Direct support for basic laboratory plasma-physics research has practically vanished in the United States. The number of fundamental investigations of plasma behavior in research centers is small, and only a handful of universities receive support for basic research in plasma physics. A striking example is the minimal support for basic research in laboratory plasmas by the National Science Foundation. . . . Because fundamental understanding of plasma properties precedes the discovery of new applications, and because basic plasma research can be expected to lead to exciting new discoveries, increased support for basic research in plasma physics is strongly recommended.

Support for basic plasma experimental research can be expected to serve an important educational function as well. University-scale experimental research programs in basic plasma science provide an excellent opportunity to train students in a variety of disciplines and techniques that are of importance in modern science and technology.

The chapters in Part II describe plasma physics experiments relevant to low-temperature and nonneutral plasmas, beams and radiation sources, and space and fusion plasmas. While many of these experimental studies have contributed significantly to our understanding of basic plasma science, they were often constrained by programmatic goals and by the plasma devices and plasma regimes relevant to a particular application. In this chapter, we focus specifically on what we have termed basic plasma experiments, whose primary goal is to isolate and study fundamental plasma phenomena in the simplest and most flexible situation possible. The objective of these experiments is to test our understanding of fundamental plasma phenomena, quantitatively and over the widest possible range of relevant plasma parameters. Although these experiments are not intended to focus directly on any particular application, they can be expected to provide a quantitative understanding of the underlying physical principles and to have a potentially significant impact on an entire spectrum of applications ranging from plasma processing and fusion to astrophysics.

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[1] National Research Council, *Plasmas and Fluids*, in the series *Physics Through the 1990s*, National Academy Press, Washington, D.C., 1986.

Experiments on plasmas in the laboratory began in the 1830s with the work of Faraday to study the role of gas discharges in the chemical transformation of the elements. Further progress hinged on the discovery of the electron and the development of the atomic theory of matter at the end of the last century. In the 1920s, Irving Langmuir discovered the existence of collective oscillations in gas discharges. The understanding of plasma-related phenomena grew substantially with studies of electron beams in the 1940s and 1950s, in conjunction with the development of beam-type microwave devices. Since then, an enormous amount of work has been done in this area, and listing all of it is beyond the scope of this report. To convey the importance of a healthy and vital effort in basic experimental plasma science, we briefly review significant accomplishments in this area since 1980. We then proceed to discuss a number of important areas in which progress could be made in the next decade. These include topics that can be expected to have broad impact in virtually all of the areas of plasma science described elsewhere in this report. By the same token, basic experiments in specific topical areas are described in Part II. Examples include studies of electromagnetic wave-plasma interactions in the chapters on radiation sources and inertial confinement fusion and studies of fluid turbulence and transport in the chapter on nonneutral plasmas.

OVERVIEW OF RECENT PROGRESS

In this section, the panel presents a selection of areas and topics in which there has been significant progress recently both in experimental studies of fundamental plasma phenomena and in the development of new experimental capabilities.

Basic Plasma Experiments

Wave Phenomena

Bernstein Waves. Bernstein waves are predominantly electrostatic waves that propagate in a magnetized plasma. These waves require a kinetic description, since the dispersion relation is dominated by the cross-field motion of the plasma particles and the wavelengths of these waves are comparable to the gyroradii of the particles. There are branches of the Bernstein wave dispersion relation associated with each of the harmonics of both the electron and the ion cyclotron frequencies. Unlike acoustic and electromagnetic waves, Bernstein waves have no analogue in uncharged fluids, and they are therefore uniquely a plasma phenomenon. They are sensitive to kinetic effects and can be used as a diagnostic of plasma behavior as well as for plasma heating. An extensive body of knowledge has now been obtained from experiments that have used a variety of antennas and boundary conditions to elucidate the unusual properties of these modes. A wide variety of linear and nonlinear phenomena that involve Bernstein waves has been explored in the last decade, and they continue to be an important topic for basic research. Results from such studies have been used to interpret satellite observations of space plasmas. This knowledge has also been used to develop schemes for heating plasmas and for diagnosing plasma behavior. For example, there are potential applications using these waves to improve the stability and confinement properties of tokamak plasmas. However, an improved understanding of the nonlinear behavior of large-amplitude Bernstein waves will be required for such applications.

Mode Conversion. Understanding mode conversion has been an important area of investigation in the last decade. In finite-temperature, spatially nonuniform plasmas, there can be degeneracy in the wave dispersion near plasma resonances, and mode conversion can occur near the spatial locations of these resonances. In particular, long-wavelength waves, which are often electromagnetic in character, can convert into electrostatic waves that then convect away the wave energy. Consequently, mode conversion can provide an important physical mechanism for absorption of the energy of electromagnetic waves. A variety of cases have now been studied, including the conversion of electromagnetic waves to Bernstein waves, Langmuir waves, lower and upper hybrid waves, and whistler waves. However, several important issues remain to be addressed. For example, although the linear transfer of energy has been observed, quantitative studies of the converted waves, the efficiency of energy transfer, and the associated electric field patterns have yet to be done, and theories of these phenomena have yet to be tested quantitatively. Understanding mode conversion is of great practical importance because of potential applications to plasma heating and use in plasma diagnostics.

Wave-Particle Interactions

Magnetically Trapped Particle Instabilities. The ubiquitous spatial nonuniformities of magnetic fields in laboratory and naturally occurring plasmas can cause the generation of two distinct populations of plasma particles: passing particles and mirror-trapped particles. Under very general conditions, the bounce motion of the trapped particles can result in the spontaneous amplification of various plasma modes. Recent experiments, based on an arrangement of multiple mirrors, have now elucidated the fundamental nature of these processes.

Lower Hybrid Wave Current Drive. Lower hybrid wave current drive, a fundamental Landau-damping process, describes the transfer of the momentum of traveling waves, which have been excited by an external source, to the momenta of the individual plasma particles. By choosing an appropriate wave, it is possible to induce a dc current in the plasma by trapping particles in the wave. The first experiments were done in a linear device. Subsequent toroidal experiments have investigated this interaction in detail by exploiting the unusual properties of lower hybrid waves. Efficient methods of current drive will be important in developing a steady-state fusion reactor.

Beat Wave Excitation and Particle Acceleration. Basic laboratory experiments have demonstrated that when a plasma is irradiated by two electromagnetic waves whose frequency difference matches the local plasma frequency, very intense (GeV per centimeter) electric fields can be generated that travel at a significant fraction of the speed of light. Recently, it has been demonstrated in the laboratory that the controlled acceleration of a tenuous electron beam can result from its interaction with these plasma waves. Such studies suggest that compact particle accelerators based on this principle may be feasible. (See Figure 5.2.)

Nonlinear Phenomena

Double Layers. A fundamental nonlinear structure encountered in plasmas is the internal nonneutral sheath or double layer. A double layer can be thought of as the boundary between regions of plasmas having different particle distribution functions. An impressive body of experimental data has now been gathered from laboratory experiments on the shape, amplitude, and formation of these remarkable structures. These phenomena are important in space science. There are indications from satellite observations that double layers may form spontaneously in the near-earth plasma. The possible relationship between double layers and the formation of auroral beams is also being investigated.

Ponderomotive Forces and the Filamentation of Electromagnetic Radiation. The ponderomotive force is one of the basic nonlinear effects governing plasma behavior. This force can be thought of as arising from the added plasma pressure produced by the oscillatory motion of charged particles in a strong electromagnetic field. When the amplitude of this field varies as a function of position, the spatial variation in this additional contribution to the pressure results in the ponderomotive force. Several experiments have elucidated the macroscopic nature of the ponderomotive force, the limits of fluid-like response, and the limitations set by the requirements for adiabatic behavior. A variety of experiments in magnetized plasmas have explored how to use the ponderomotive force to quench various configurational instabilities and thereby to produce quieter and longer-lived plasmas with improved particle and energy confinement.

Experiments have been conducted to study the propagation of a high-power laser beam through a plasma and the resulting plasma response. These experiments have demonstrated the filamentation of the primary beam into high-intensity beamlets, which trigger secondary plasma-wave instabilities and create associated beams of fast electrons.

Magnetic Field Line Reconnection. The first magnetic field line reconnection experiments were done more than a decade ago in plasma pinch devices. Recently, a new generation of precise and well-controlled laboratory experiments has been carried out in which the ions are effectively unmagnetized but the electrons are magnetized. The magnetic field topology was mapped in three dimensions, and its dependence on plasma parameters was investigated. Observations include Alfvénic ion flow from the neutral sheet (i.e., a plane in the plasma at which the local magnetic field vanishes) and the formation of a neutral sheet on time scales less than the Alfvén transit time across the sheet. In the case where the current sheet was much narrower than its length, the breakup of the current sheet into a filamentary structure was observed. Other important nonlinear and three-dimensional phenomena were observed and studied, including the spontaneous generation of whistler-wave turbulence, the local formation of double layers, the generation of magnetic helicity, and the observation of highly non-Maxwellian particle distribution functions.

Recent experiments have also studied magnetic reconnection in the merging process that occurs when two spheromak plasmas are brought together. (See Figure 8.1.) These plasmas are isolated structures, spheroidal in shape, that are self-sustained by a combination of currents and magnetic fields. In this case, there are local current sheets with magnetized ions. These experiments indicate that the merging process depends qualitatively on the initial helicities (i.e., the "twists") of the magnetic fields of the plasmas involved in the merger process.

Plasma Reorganization. Several experiments have been done in the past five years on the merging of plasma currents and the propagation of currents across magnetic fields. (See Figure 8.2.) One common feature of these experiments is that the current flows are fully three-dimensional. For example, merging currents in a high-beta plasma (i.e., a plasma in which the plasma pressure is comparable to that provided by the confining magnetic field) were observed to spiral about each other as they coalesced. The currents evolved to become nearly parallel to the local magnetic field and hence force free. An elegant experiment in which an electron current was made to propagate across a magnetic field showed that whistler waves played a key role in the evolution of the current channel.

The experiments relied on highly reproducible, repetitive, plasma sources and on probes capable of studying the three-dimensional nature of the plasma behavior. These experiments are relevant to space plasma physics (such as the tethered shuttle experiment), solar physics, the study of helicity generation and helicity injection, and the behavior of three-dimensional current systems.

Chaos and Turbulence

Chaos. Accurate description of the plasma dielectric response relies on integration of the perturbation caused by an applied field along the trajectory of plasma particles. The perturbed currents and densities thus obtained may then be put into Maxwell's equations to determine the wave dispersion. However, even in a uniform magnetized plasma, the application of a single, finite-amplitude plane wave can be sufficient to render the particle orbits chaotic, and no self-consistent theory exists for the plasma dielectric response in this case. Experiments have now determined that non-self-consistent chaos theory correctly predicts several aspects of wave-induced particle chaos, as long as the wave amplitude is sufficiently small. Conservation laws describing the particle orbits, even during chaotic particle motion, have also been identified. Chaotic heating of plasmas has been observed, not only from externally launched waves but also from spontaneous, unstable waves in a plasma that is externally driven. These experiments were made possible by laser-induced fluorescence techniques that have advanced dramatically in the last decade.

Quasilinear Effects and Single-Wave Stochasticity. A series of experiments in single-component electron plasmas, which were carefully designed to eliminate the complications arising from ion dynamics, have tested the fundamental assumptions of "quasilinear theory," the standard model of weak plasma turbulence. These experiments demonstrated the importance of mode-coupling effects in modifying the wave-particle interactions described by the theory. In particular, in the presence of a mildly nonmonotonic particle distribution, unstable waves were found to grow and then saturate at the level predicted by the theory. However, the growth rates of individual waves were found to depend on the rates at which other waves grew, and this is *not* accounted for in the theory. Thus, a complete understanding of this important problem has yet to be achieved. This topic is related to the common assumption of the "random phase approximation" in turbulence theory, which is central to current descriptions of weak turbulence. The potential for new experiments in this area is discussed below in the context of turbulence and turbulent transport.

Complementary experiments have observed the evolution of a large-amplitude monochromatic wave to a stochastic signal, via sideband generation and trapped-particle dynamics. A very important, but as yet unresolved, question is the detailed mechanism by which a single, large-amplitude wave is transformed into the background of weak turbulence that can be addressed by quasilinear theory.

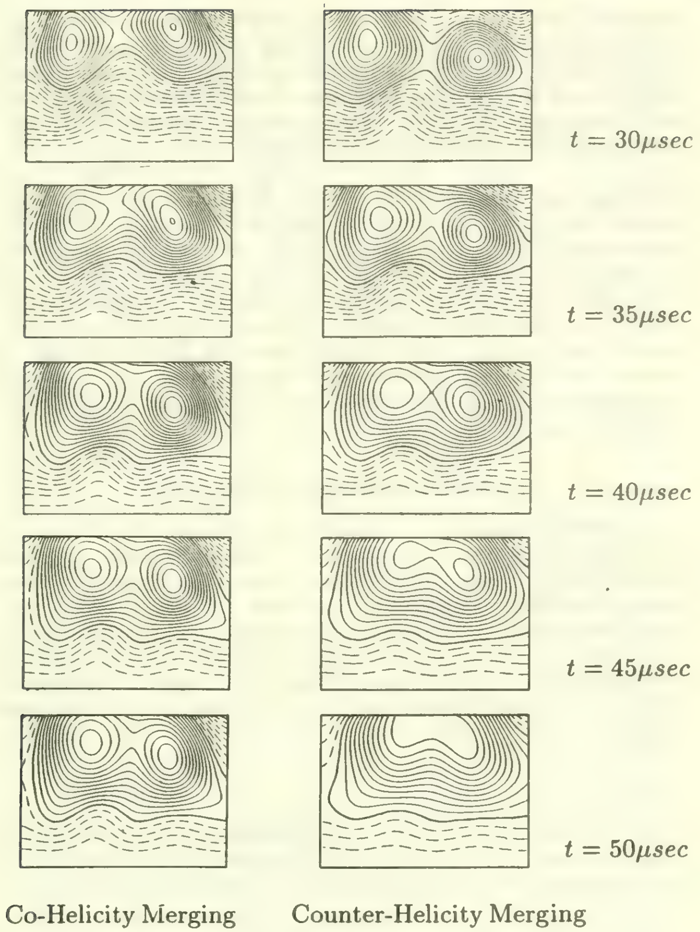

FIGURE 8.1 Experimental study of magnetic reconnection processes in the merging of two spheromak plasmas. This experiment demonstrated that the three-dimensional structure of the magnetic field is crucial to the merger process in that the difference between the co-helicity and counter-helicity merger process is due to the relative directions of the out-of-plane components of the magnetic field in the two plasmas. A new mechanism of

Collisionless Heat Transport. Laboratory experiments have now explored the important question of how heat is transported in collisionless plasmas. These measurements involve the application of high-power microwave beams to generate hot electron tails in a nonuniform plasma. The qualitative features of this effect and the important scaling properties have been identified. They have helped to clarify the relevant theoretical issues in this area.

Strong Langmuir Turbulence. One of the significant advances in the understanding of nonlinear plasma behavior has been the development of the concept of plasma-wave collapse and the associated spiky turbulence that frequently accompanies it. Several laboratory experiments, aimed at uncovering the microscopic dynamics of Langmuir-wave collapse, have used both electromagnetic driving and electron beams to trigger the collapse of extended wave packets, which in turn produces strongly localized fields and density depletions or cavitons. More recently, the ionosphere has been used to demonstrate the ubiquitous nature of these phenomena and the important role they play when a plasma is driven by large-amplitude perturbations.

Experimental Techniques and Capabilities

Opportunities for advances in experimental physics are often linked to the development of new technologies. The effect of these technologies is twofold. First, they enable the creation of experimental conditions that permit the demonstration and isolation of important physical effects. In plasma science, this frequently involves both new means of plasma production and new means of plasma confinement. In addition, new technologies frequently lead to new diagnostic techniques and new means of processing data, which not only result in improved accuracy and precision but often result in new perspectives on the underlying physics.

Plasma Sources

Over the past 20 years, there has been substantial progress in the development of improved, quiescent plasma sources. Some of the first, "high-quality" plasmas used in basic research were created in Q machines (Q stands for "quies-

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plasma acceleration (perpendicular to the plane of the figure) was discovered in the course of this work. (Reprinted, by permission, from M. Yamada, F.W. Perkins, A.K. MacAulay, Y. Ono, and M. Katsurai, Physics of Fluids B 3:2379, 1991. Copyright © 1991 by the American Institute of Physics.)

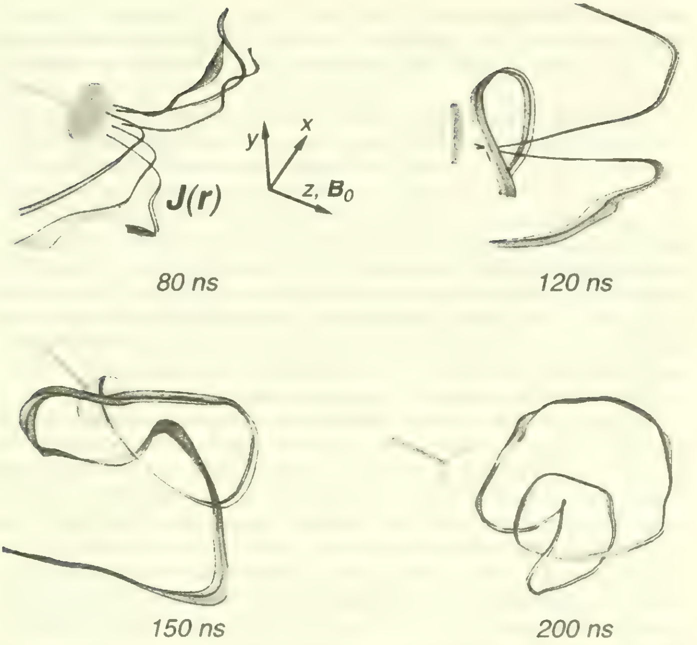

FIGURE 8.2 Experimental study of the penetration of a pulsed current into a magnetized plasma. Shown are the characteristic field lines, sheets, and tubes of the current density, $J(r)$, at different times after a 100-ns (FWHM) current pulse is applied to a disk electrode (shown). These data are extracted from a dataset of 10,000 point measurements at each time step. Typical experiments involve studying 1000 such time steps. At 80 ns, the current penetrates a short distance from the positive electrode into the plasma, before turning back to the negative electrode located at the back endwall of the vacuum chamber. Little helicity is observed in this fountain-like current flow. As the current propagates (120 ns) two distinct current systems are observable: a closed azimuthal Hall current in regions where $J_z \approx 0$ and field-aligned solenoidal plasma currents between the positive and negative electrodes. At 150 ns a current tube starts off-axis, where $J_z \neq 0$ and $J_B \neq 0$, and exhibits strong helicity; i.e., it twists and knots in the right-hand direction. After the end of the applied current pulse, at 200 ns, the current lines detach and propagate away from the electrodes, and shown is a closed, singly-knotted, twisted current tube. Experiments like this, which illustrate the fully three-dimensional nature of the dynamics of the plasma response resulting from such a current pulse, have recently been made possible by the advent of fast, relatively inexpensive laboratory computers with large data handling capabilities. (Courtesy of R. Stenzel and M. Urrutia, University of California, Los Angeles.)

cent"), which were developed 30 years ago. These devices generate a magnetized plasma column, with a diameter of about 10-20 ion Larmor radii, that is well suited for the study of such phenomena as drift waves and ion cyclotron modes.

The plasmas in Q machines are such that the electrons and ions have equal temperatures (i.e., $T_e = T_i$). Consequently, these devices are not appropriate for the study of ion acoustic waves, which are strongly damped in such plasmas. The use of large numbers of small, permanent magnets to produce surface magnetic confinement, together with a variety of different electron sources, has provided a way to produce unmagnetized, collisionless plasmas that are both isotropic and quiescent. Such devices have $T_e/T_i \approx 10$, and they are well suited to the study of the linear and nonlinear behavior of ion acoustic waves. These plasma devices have also permitted experiments on plasma sheaths and on a variety of other linear and nonlinear waves. Combination of two or three of these plasmas has resulted in so-called double and triple plasma devices that have been used to study beam-plasma interactions, solitons, and electrostatic shocks.

In the past decade, dc discharges based on oxide-coated cathodes have resulted in the ability to produce large, quiescent, magnetized plasma columns, of the order of 50 cm in diameter (which is equivalent to 500 ion Larmor radii) and 10 m in length. Efficient, microwave-generated plasmas are now also conveniently available. Electron cyclotron resonance sources provide another way to study highly collisional plasma phenomena, with ion-neutral mean free paths of several centimeters or less. Inductive sources have recently shown considerable promise in producing uniform, unmagnetized and magnetized plasmas in the pressure range greater than 5 mtorr and, for example, have already been employed in studies of double layers.

During the last few years, "helicon" sources (bounded whistler-wave sources) have produced steady-state plasmas with densities as high as 10^{14} cm^{-3}. Such sources, which operate between the lower hybrid and the electron cyclotron frequency, do not have a high-density cutoff; they are therefore useful in producing plasmas with high densities.

Plasmas consisting of negative and positive ions, with very low concentrations of electrons, have also been created, both with and without a magnetic field. The production of these plasmas relies on the large electron-attachment coefficient of gases such as SF_6 for cold electrons. For sufficiently low values of the electron density, waves and instabilities in these plasmas can differ qualitatively from those in electron-ion plasmas, since the dominant charge species now have comparable masses.

Mechanical Probes

Refinement of probe techniques has occurred hand in hand with plasma source development. These include directional velocity analyzers (with resolutions of a few degrees in velocity space), emissive probes, and electric dipole probes that are sensitive to the total electric field, including the magnetic component (i.e., $E = -\nabla\Phi - \partial A/\partial t$). Computer-controlled probe drives capable of full, three-dimensional motion have been developed, and essentially the first-ever fully three-dimensional studies of several important plasma phenomena have been conducted.

Laser-Based Optical Diagnostics

The potential of laser-based optical diagnostics for plasma science has continued to develop. Two general categories of recent achievements are measurements of the spectrum of collective plasma density fluctuations and measurement of the single-particle distribution function, both by Thomson scattering and by laser-induced fluorescence. Scattering from collective plasma fluctuations has been used to study both thermal and nonthermal effects, such as waves and instabilities and entropy fluctuations. A variety of imaging diagnostics, such as phase-contrast and shearing-plate interferometry, has been developed for use in experimental plasma research. Single-particle scattering techniques have been developed to study both the ion and the electron velocity distributions. Thomson scattering techniques have been developed to measure electron density and temperature with unprecedented spatial and temporal resolution.

The use of laser-induced fluorescence techniques to study plasma ions has been one of the major recent developments in plasma diagnostics. Not only does this technique permit temporally and spatially resolved measurement of the ion distribution function with high sensitivity, but it also permits a variety of extensions. (See Figure 8.3.) For example, the use of metastable states provides the capability of measuring nonlocal plasma properties, and metastable or spin-polarized ions produced by optical pumping may be used as test particles to trace ion orbits and to study particle transport.

Laser-induced fluorescence has now been used to measure ion distribution functions in a variety of physical processes and to study the plasma dielectric response, and optically tagged test particles have been used to measure Fokker-Planck coefficients for collisional diffusion. Such test particles have also been used to measure the Lyapunov exponents that characterize chaotic particle motion and to measure the transport arising from a variety of physical processes.

Recent developments in laser and optical technologies include the development of new, high-performance laser materials, such as Ti-sapphire, and a wide variety of solid-state lasers. Other important developments include nonlinear optical materials, which are essential to the production of tunable radiation at short wavelengths via frequency multiplication. Improvements in short-pulse laser technology have increased the time resolution of measurements to better than 10^{-14} s, which is shorter than virtually all of the natural time scales in most laboratory plasmas.

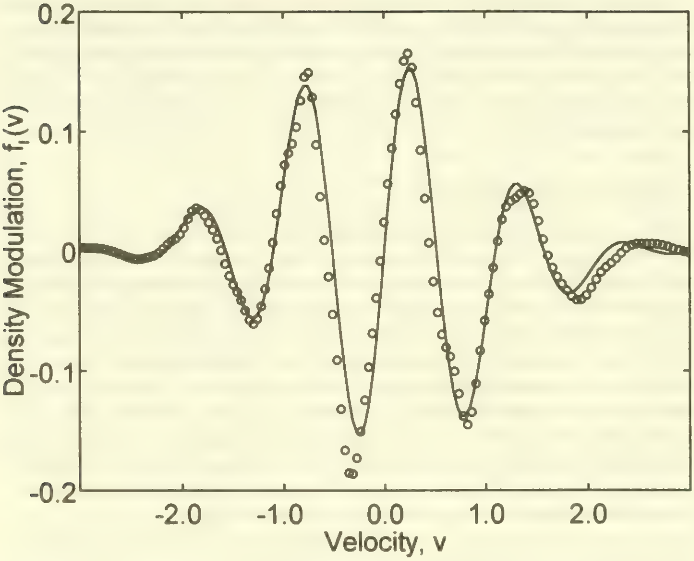

FIGURE 8.3 The oscillatory part of the ion velocity distribution, associated with a wave in a magnetized plasma, is shown as a function of the velocity of the ions. The data (open symbols) were measured nonperturbatively at a point in the plasma with recently developed laser-induced fluorescence techniques. For comparison, the solid curve shows the theoretical prediction, assuming an ideal plane wave. Experimental techniques such as the one illustrated are now capable of directly studying detailed aspects of plasma behavior, including plasma flows, chaotic particle motions in response to large-amplitude waves, and particle transport due to waves and turbulence. (Courtesy of F. Skiff, University of Maryland.)

Data Acquisition and Processing

Improvements in data acquisition systems and probe drives now allow the recording of fully three-dimensional data sets with good spatial and temporal resolution. Such data are now routinely displayed with relatively inexpensive, powerful workstations. The current state of the technology permits the exploration of many important physical processes that are of fundamental interest to basic plasma physics and are relevant to a variety of applications.

RESEARCH OPPORTUNITIES

Fundamental Plasma Processes

The following is a selection of important fundamental problems in basic plasma science that could be addressed in the next decade by a new generation of plasma experiments.

Wave Phenomena

Alfvén Waves. Alfvén waves are modes of oscillation of a magnetized plasma at frequencies below the ion cyclotron frequency. They are important both in fusion plasmas and in space plasmas, such as the solar wind and the Earth's magnetosphere and ionosphere. Alfvén waves can act to transport information about magnetic field disturbances in magnetized plasmas. Alfvén waves and their high-frequency analogue, magnetosonic waves, are important in magnetic confinement fusion research, where they are candidates for plasma heating and noninductive current drive.

In spite of their importance, relatively little work on these waves has been done in the laboratory because Alfvén waves have relatively long wavelengths (1-5 m) in plasmas of reasonable density. Recently, well-diagnosed plasmas have been developed that are sufficiently dense and large enough to accommodate several Alfvén wavelengths. Thus, carefully controlled laboratory Alfvén wave experiments are now possible. Topics currently under investigation include the dispersion relation for these waves, their reflection properties and spatial structure, and the nonlinear behavior of these modes.

Wave-Plasma Interactions. Wave-plasma interactions have been under active investigation, but many important questions remain. Outstanding issues include the modulation of plasma currents by waves and the modulation of low-frequency waves (e.g., Alfvén and whistler waves) by local fluctuations in the plasma conductivity. Another important problem that has not yet been studied experimentally is resonant absorption in the situation where a wave propagates along a density gradient that is parallel to the magnetic field. Important effects include nonlinear refraction and the generation of non-Maxwellian electron distribution functions and Langmuir turbulence. Such experiments are relevant to fusion physics as well as to heating of the F-region of the ionosphere. Information generated by these experiments also may be relevant to the development of advanced particle accelerators and to the generation of intense electromagnetic waves.

Intense Laser-Plasma Interactions. As described in Chapter 5 on beams and radiation sources in Part II, recent technological developments have led to the development of compact, subpicosecond terawatt lasers with beams that can be focused to intensities greater than 10^{18} W/cm^2. At these intensities, an electron is accelerated to relativistic energies in one period of the laser light. This permits the study of highly nonlinear, fast, relativistic processes in laser-plasma interactions. If the laser is focused on an overdense plasma target, dc magnetic fields of the order of 10^9 G are predicted to occur. It will be important to determine whether the nonlinear ponderomotive forces and relativistic effects can reduce the diffraction of these ultrahigh-intensity laser beams, so that the light can be focused to beam sizes smaller than a few Rayleigh lengths, the limit expected at lower values of light intensity and in a linear medium.

Chaos, Turbulence, and Localized Structures

Nonlinear Particle Dynamics and Chaos. Modern concepts of nonlinear dynamics have created a renaissance in classical physics, bringing new techniques to bear on long-standing problems. One crucial issue in plasma physics is the onset of chaotic particle motion in response to coherent or turbulent wave fields. Of particular interest are a self-consistent description of the system under such circumstances and the evolution of the system from regular particle motion to chaos. It is now possible to conduct precisely controlled experiments in the laboratory to address these important problems, which are of interest in a wide variety of contexts, ranging from fluid dynamics to advanced particle accelerators.

Nonlinear Wave Phenomena. With the exception of Alfvén waves, most of the other linear branches of the plasma dispersion relation have been explored. In addition, many nonlinear, three-wave coupling processes have been observed. However, the transition from linear to turbulent wave behavior is not understood. This includes the nonlinear behavior associated with almost every branch of the plasma dispersion relation.

Turbulence. Very generally, plasmas are electrodynamic, many-body systems far from equilibrium that are dominated by nonlinear effects. Consequently, plasmas are typically highly turbulent, exhibiting large fluctuations in such quantities as the local density, temperature, and magnetic field, which can vary rapidly in time and space. Important examples of plasmas whose behavior is influenced profoundly by turbulence include essentially all magnetically confined fusion plasmas and many astrophysical and space plasmas. We have no first-principles understanding of turbulence in *any* plasma, and understanding such turbulent behavior is perhaps the key unsolved problem in plasma physics. This problem presents an important synergism with fluid dynamics, in that plasmas can often be modeled as fluids and understanding turbulence is central to a complete description of fluid systems.

Since turbulence is so common in plasma physics, the potential rewards for achieving predictability are particularly high. In the past decade, new plasma sources and measurement techniques have been developed that will allow us to undertake a new generation of precisely controlled experiments to study turbulent plasma behavior. One starting point in achieving a deeper understanding of turbulence will be further study of the questions raised by the observed breakdown of quasilinear theory and experimental tests to determine the range of validity of the random phase approximation.

Turbulent Transport. Profound consequences of plasma turbulence include the transport of both particles and energy and the acceleration of particles that can be induced by turbulence. Such transport can completely dominate plasma behavior. For example, transport by turbulence, in the form of both convection and enhanced diffusion, is the dominant transport mechanism of particles and energy in present-day tokamak fusion plasmas. Turbulent transport presents an excellent opportunity for carefully controlled laboratory experiments. At least in low-temperature laboratory plasmas, techniques are now available to study both the transport of particles and energy and the fluctuations responsible for this transport. To establish the causal connection between turbulence and transport, it will be necessary to make precise, spatially resolved measurements of fluctuating quantities such as plasma temperature, density, velocity, and magnetic field and to establish the correlations between these quantities and local measurements of the particle and energy fluxes.

Because turbulence and turbulent transport are not understood in any plasma, careful experimentation in flexible, small experiments is likely to make significant contributions to testing existing theoretical predictions and to guide further theoretical work in this important area. Given the fundamental lack of understanding and the important practical consequences that would derive from a deeper understanding of turbulence and turbulent transport, a sustained program of both theoretical and experimental research is extremely important.

Sheaths, Boundary Layers, and Double Layers. Plasma sheaths (i.e., regions where the plasma is not charge-neutral) have been an important topic throughout the history of plasma physics. All plasmas in the laboratory and in space have boundaries at which there are sheaths, and probes and antennas immersed in plasmas are surrounded by such sheaths. One important area for future research is the nature of sheaths in magnetized plasmas. To probe the structure of such sheaths requires detectors smaller than an electron Debye length. Such probes and probe arrays can be expected to be available in the next few years.

Double layers are a class of sheaths that are detached from a physical boundary and are supported by locally non-Maxwellian conditions. Although there has been some research done on double layers, there has been little work on situations in which the ions are magnetized and situations involving the transition regime between collisionless and collisional plasmas. This is of importance for solar physics (in regard to coronal holes) and in ionospheric heating experiments. Double layers are responsible for localized particle acceleration and could also play an important role in the aurora.

Shock Waves. Much laboratory work has been done on shocks in unmagnetized plasmas. Shocks also have been studied in pinches and exploding wires. However, careful experiments on Alfvénic shock waves in magnetized plasmas have yet to be done. Of particular interest is the propagation of large-amplitude (i.e., $\delta B \approx B$) magnetic pulses. Work would include studies of wave steepening, particle reflection and heating, and a search for a new class of shocks (the "intermediate shock") that has been predicted but not yet observed. Such shock wave phenomena are of importance in space and astrophysical plasmas.

Striated Plasmas. Plasmas with nonuniformities, such as density or temperature striations in the direction parallel to the magnetic field, are of fundamental interest. They occur, for example, in the ionosphere and in the aurora. If the gradient in the plasma properties is steep compared to the wavelengths of interest, these structures can trigger the mode conversion of whistler waves. The reflection, refraction, and interaction of waves with plasma structures that have steep gradients has not been studied in the laboratory and presents a difficult "plasma scattering" problem. Striated plasmas are not limited to those with density perturbations but also include local "hot spots" and magnetic field perturbations. Topics of interest include the interaction with lower hybrid waves, refraction and reflection, fast-particle generation, and minority-species heating.

Flows in Magnetized Plasmas. It now is possible to generate highly magnetized laboratory plasmas in which the diameter of the plasma column is much larger than the ion Larmor radius (e.g., by factors of as much as 10^3) and in which magnetic Reynolds numbers of 10^4 to 10^5 are attainable. (The magnetic Reynolds number is the time scale for transport of the magnetic field by the flowing plasma relative to the time scale for diffusion of the field due to the finite resistivity of the plasma.) By using multiple sources (so called "double-plasma" configurations), flowing plasmas can be generated with drift velocities comparable to the Alfvén wave velocity and with Mach numbers (i.e., plasma flow velocities relative to the ion sound speed) of the order of 500. Large currents can be entrained in these plasmas. Such situations are predicted to lead to shocks and turbulence. Insights into dynamo action (discussed below) are also likely to be achieved in such experiments. These experiments also would be relevant to the solar wind and to other solar and astrophysical processes.

Plasmoids. "Plasmoids" are plasma structures that propagate as recognizable entities through a background plasma. Satellite data suggest that plasmoids may occur in the Earth's magnetotail and become detached and move away from the Sun during magnetic storms. The propagation of plasmoids has been studied with small plasma guns. Larger structures of interest to fusion physics (i.e., spheromak plasmas) have also been investigated. (See Figure 8.1.) The latest generation of diagnostics, coupled with the recently developed ability to generate plasmoids easily, now permits a new generation of experiments. For example, one now can study in detail how plasmoids are generated, how they propagate, and the details of their internal field structure and associated plasma currents.

Magnetic Effects

Magnetic Field Line Reconnection. Magnetic field line reconnection is one of the principal means by which magnetic field energy is converted to thermal energy and plasma motion. For example, it is thought to be responsible for the high temperature of the solar corona and to be important in the Earth's magnetotail and in many astrophysical situations. Magnetic reconnection also is of importance in understanding the so-called sawtooth crashes that occur in the hot core of tokamak plasmas when a certain type of magnetohydrodynamic instability is present. In this case, reconnection has the effect of expelling hot plasma from near the plasma center and is detrimental to plasma confinement.

Experiments on magnetic reconnection in plasmas with unmagnetized ions and magnetized electrons have already been done. Areas for further study include cases where the magnetic Reynolds number is greater than 100. Important issues include the three-dimensional nature of this phenomenon, the connection between global and local time scales, the acceleration and heating of the plasma particles, and the generation of plasma flows.

Dynamo Action. The dynamo is a process by which the kinetic energy of a conducting fluid is transformed into magnetic field energy. (See Figure 8.4.) In a dynamo, a "seed" magnetic field from a small current fluctuation can be stretched and reconnected by the turbulent fluid motion. In principle, this can lead to amplification of the magnetic field to a level where the magnetic field dominates the dynamics of the fluid flow. The dynamo is a fundamental process in magnetohydrodynamics, and dynamo action is crucial to understanding many aspects of space physics and astrophysics. For example, it is believed to be the origin of the magnetic fields of such diverse objects as the Sun and the accretion disks of stars and is intimately connected with the physics of novae and supernovae.

The conditions for dynamo action require large-scale flows in highly conducting media, and up until now, such conditions have proven difficult to achieve in the laboratory. The criterion for dynamo action is the achievement of Reynolds numbers on the order of 100. Possibilities now exist to carry out well controlled

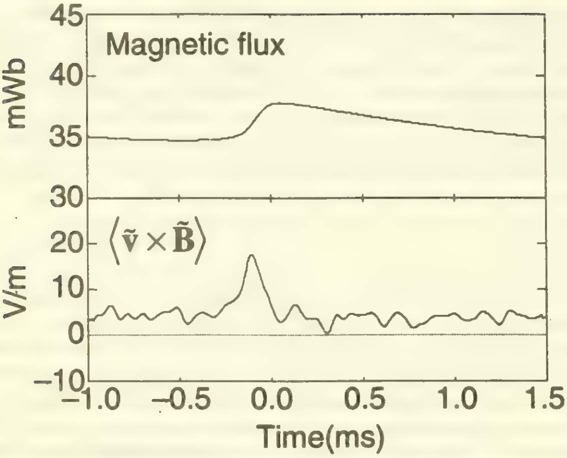

FIGURE 8.4 Demonstration of the dynamo effect in a laboratory plasma. The dynamo effect is the spontaneous self-generation of magnetic fields within plasmas, a process common in astrophysical bodies such as the Sun. Shown is a "dynamo event" in which magnetic flux is suddenly generated in a toroidal laboratory plasma. By measuring the local electromotive force, $\langle v \times B \rangle / c$, generated by fluctuations in plasma velocity and magnetic field, it was established that this force is responsible for generation of the magnetic flux. The fluctuating flow and field were measured with Langmuir and magnetic probes inserted into the plasma edge. This particular dynamo mechanism has long been thought to be an important source of astrophysical magnetic fields. (Reprinted, by permission, from H. Ji, A.F. Almagri, S.C. Prager, and J.S. Sarff, Physical Review Letters 73:668, 1994. Copyright © 1994 by the American Physical Society.)

dynamo experiments, for example by establishing a rapid fluid flow in a large vat of liquid sodium. Such experiments can provide new and fundamental insights into the nature of dynamo action and provide a quantitative basis for refined theories of many important physical processes.

Magnetic Reconfiguration. Many plasma instabilities and processes are inherently three-dimensional. Laboratory experiments now allow these processes to be explored in detail. Important topics include the dynamics of three-dimensional current systems, current and wave filamentation, current sheet formation, the effect of magnetic forces on plasma currents, helicity generation, and helicity conservation. These processes have far-reaching consequences in the understanding of solar flares, solar magnetospheric physics, and fusion physics.

New Experimental Capabilities

In any experimental science, and particularly in physics, advances in diagnostics consistently have led to new discoveries and frequently have opened up entirely new areas of research. Several new tools for plasma research are now becoming available. Some of these techniques have not been developed with plasma diagnostics in mind, but they can be expected to have significant impact on experimental plasma science. In many cases, progress is likely to require the collaboration of plasma physicists, solid-state physicists, and engineers. Much of this work will have important applications in other fields.

Use of Nanotechnology

Advances in nanotechnology are likely to have a profound impact on experimental plasma physics. Typical devices are miniature valves and mass analyzers. Techniques widely used in the semiconductor industry will enable the production of particle detectors, mass-sensitive energy analyzers, and magnetic and electric field probes with overall scale sizes smaller than 1 mm and active sensor areas less than 10 mm in diameter. These detectors will be capable of providing spatially resolved measurements on the scale of the Debye length and electron cyclotron radius in research plasmas with densities of the order of 10^{12} cm^{-3}, electron temperatures of tens of electron volts and magnetic fields of 0.1 T. Such probes would produce a minimal perturbation of the plasma if their connections and supports were also microscopic (≈ 0.2 mm). It is likely to be possible to position many (e.g., 10^4 to 10^6) of these detectors on a lattice that could be moved within the plasma.

Optical Diagnostics

The recent discovery of giant Faraday rotation in magnetoactive crystals now enables the construction of magnetic field probes as small as 10 μm in diameter and less than 1 mm long. As a light beam traverses one of these small crystals, the plane of polarization of the light is rotated. Preliminary tests have demonstrated sensitivities of 1 G per degree of angular rotation and response times faster than 10^{-9} s. Such probes are immune to electrical pickup and are nonmetallic. Another important capability has been created by the discovery of the quantum-well effect in crystals. Quantum-well devices can now be fabricated into microscopic probes to measure the local amplitude of the electric field. Arrays of these optical probes could be used to diagnose the space-time behavior of the electric fields associated with plasma waves and currents.

New Regimes of Plasma Parameters

As described in Part II, advances in laser technology now make possible laboratory experiments in previously inaccessible regimes of plasma parameters. Both short-pulse, high-power lasers and multiphoton ionization using tuned sources can be used to produce liquid or solid density plasmas, in which both quantum and classical many-body effects are important. The creation of these high-energy-density plasmas also opens up the possibility of studying plasmas with highly ionized ions (i.e., high-Z plasmas).

Data Acquisition

In the past 10 years, experimental physics has benefited greatly from advances in digital technology. Analog-to-digital converters and microprocessors have decreased drastically in price. Workstations are now available with 128 Mbyte of memory and 8 Gbyte of disk storage, and this trend shows no sign of saturating. A system with 10^6 channels of acquisition is capable of acquiring on the order of 1 Gbyte of data per second. Such a data acquisition system might consist of many parallel processors sharing a fast network and have 10 Gbyte of random access memory and 10 to 100 Tbyte of mass storage. This system would permit the study of nonuniform and fully three-dimensional plasma phenomena and plasma processes occurring on more than one spatial scale with unprecedented spatial and temporal resolution.

Massively Parallel Plasma Diagnostics

Interactions among plasma particles range from short-range collisions between individual particles to long-range, collective forces; consequently, plasmas frequently contain several different characteristic length scales. Magnetized plasmas are inherently anisotropic and nonlinear, exhibiting nonlocal behavior, chaotic particle motions, turbulence, and self-organization. The fundamental equation describing the plasma behavior of a many-body system of N charged particles is Liouville's equation for the distribution function in the $6N$-dimensional phase space of the system. As a practical matter, theoretical descriptions of plasmas are frequently based on much simpler and more tractable equations. However, the assumptions concerning the statistical structure of the plasma, which buttress the derivations of these simpler treatments, have not been tested.

In the next decade, a new generation of plasma experiments is likely to be able to make significant contributions in testing the validity of the approximations used to describe plasmas, for example, as fluids or as many-body systems described by kinetic theory or by a particular kind of particle correlation function. With what is now or will shortly become available, experimental plasma science will be able to explore a range of plasma problems with a precision and to a degree of detail previously unattainable. Below, we briefly list some possibilities, assuming the capability exists to create a lattice of thousands of microscopic detectors and/or thousands of channels of optical probes.

In plasma physics, the plasma is typically described by a combination of time-averaged and fluctuating fields. In the case of a turbulent plasma, the average of a quantity may be much smaller than its fluctuating component. If one considers an experiment that is repeated many times, all individual quantities, such as the instantaneous values of the electric and magnetic fields, the density, and the particle distribution function, can in principle be measured and recorded. This detailed set of measurements could be used to calculate the higher moments of the distribution function to test the assumptions that go into the derivations of the equations of kinetic theory. For example, our present understanding of three-body correlations is poor, but such quantities could be measured directly.

Fine structure in the particle distribution functions could also be measured. Measurements by particle detectors on spacecraft and in the laboratory indicate that when instabilities are present, the distribution functions cannot be regarded simply as functions of the magnitudes of the components of velocity perpendicular and parallel to the magnetic field. Snapshots of the distribution functions could be expected to reveal phase-space structures that go far beyond such a simplified description. Such highly anisotropic particle distribution functions can be expected to have profound effects on the growth and damping of a variety of plasma waves. With such detailed measurements, one could also test the validity of equating temporal averages with spatial ones.

The ability to probe fine spatial scales will permit a detailed exploration of plasma sheaths and boundary layers. For example, tiny puff valves and microbeam sources could be used to tailor the local particle distribution function or to add minute quantities of an impurity ion. Finally, phenomena on scale lengths ranging from less than the Debye length, to the ion cyclotron radius, to the electron cyclotron radius, could be explored simultaneously in one experiment. This would allow exploration of physics from the regime in which single-particle interactions are important to the regime in which kinetic and MHD effects are dominant.

SUMMARY, CONCLUSIONS, AND RECOMMENDATIONS

The panel has great concern that basic experimental plasma science is disappearing in the United States. By its count, there are currently fewer than 20 groups engaged in basic plasma experiments in the United States. Yet an intellectual atmosphere that allows for dialogue, complementary experiments, and in some cases, competition is necessary for any field of modern science to make efficient progress. There is tremendous benefit to be derived if different research groups working on similar and complementary problems can exchange ideas and collaborate. Investigators free to follow where their research leads produce qualitatively new insights and new approaches to the underlying science. This type of scientific environment typically produces new techniques and new ideas on a rapid time scale. Such basic science is the foundation for applied science. These processes do not happen as frequently at large "technology centers," which must operate in a less flexible and more programmatic fashion.

In plasma science, the place for new and significant discovery is very frequently the laboratory. When there are significant new theoretical predictions, much of the value of these predictions is lost if they cannot be tested quantitatively by experiment. There is no adequate substitute for carefully planned and precisely controlled laboratory experiments. Many interesting and stimulating observations of plasmas can be made by spacecraft, but space experiments are not a substitute for the well-controlled and repeatable experiments that can be performed in the laboratory. The notion that computers can simulate plasmas so well that laboratory experiment can be replaced is also incorrect and is likely to remain so for the foreseeable future.

Laboratory experiments, theory and modeling, spacecraft and astrophysical observations, active space experiments, and experiments on fusion plasmas are synergistic. It is the healthy interplay among all these elements that will lead to a healthy plasma science. The field can "get along" for a while ignoring one element or the other, but it cannot continue for long in the unbalanced manner that has occurred in the last decade in the case of basic laboratory plasma experiments. Without a healthy underpinning of experimental laboratory work, the field of plasma science will not attract talented young scientists and is destined to become sterile and inefficient.

As a consequence, the highest priority of the panel is the establishment of a system of sustained support for modest-sized experimental efforts, sufficiently small and flexible that they can make rapid changes in their approach to a research problem, guided by the internal logic of the science and by new experimental and theoretical discoveries as they develop. As discussed in Chapter 4, fundamental aspects of plasma science crucial to fusion physics must be pursued in detail in the fusion-relevant geometries provided by large plasma devices and facilities. However, given the relatively high costs of operation of large facilities and the limited funds that one can expect for fundamental plasma experiments, the number of large devices not motivated by important applications such as space science or fusion is likely to remain small for the foreseeable future.

It is the conclusion of the panel that the type of sponsorship necessary for a revitalization of basic plasma experimental science is the support of at least 30 to 40 independent groups at a reasonable level for experimental research in a university, which is of the order of $200,000 to $400,000 per year. For example, funding at the $200,000 level would allow a program of two students, a postdoctoral researcher, and modest expenditures on equipment and supplies. Larger programs would require some technical support and additional personnel and equipment, as appropriate. It is important that such support be granted for periods of at least three years, given satisfactory performance, so that significant research goals can be set and accomplished. Large equipment purchases would have to be funded from separate equipment proposals. Given that the infrastructure for basic experimental facilities has declined so significantly in the past two decades, additional initial equipment purchases, where necessary, would typically range from $300,000 to $600,000 per program. Additional mechanisms that would allow for collaboration between groups on a rapid time scale, compared to the proposal cycle that now exists, would also be beneficial. Placing some resources at the discretion of program managers would be one way to accomplish this.

The increased support that the panel recommends for basic experimental research can be expected to serve an important educational function as well. It is generally recognized that small-scale experiments are an excellent setting in which to train students. The training of students, under the guidance of their supervisor, to make qualitative changes in an experiment or even in research direction as results unfold is invaluable in modern research and technological development. In addition, experimental plasma science students typically receive very thorough training in such important areas of modern technology as digital electronics, optics and computational hardware and software.

The following of the panel's general recommendations (see Executive Summary) are made to implement the revitalization of experimental plasma science described above:

1. To reinvigorate basic plasma science in the most efficient and cost-effective way, emphasis should be placed on university-scale research programs.

2. To ensure the continued availability of the basic knowledge that is needed for the development of applications, the National Science Foundation should provide increased support for basic plasma science.

3. To aid the development of fusion and other energy-related programs now supported by the Department of Energy, the Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, should provide increased support for basic experimental plasma science. Such emphasis would leverage the DOE's present investment in plasma science and would strengthen investigations in other energy-related areas of plasma science and technology.

4. Approximately $15 million per year for university-scale experiments should be provided, and continued in future years, to effectively redress the current lack of support for fundamental plasma science, which is a central concern of this report. Furthermore, individual-investigator and small-group research, including theory and modeling as well as experiments, needs special help, and small amounts of funding could be life-saving. Funding for these activities should come from existing programs that depend on plasma science. A reassessment of the relative allocation of funds between larger, focused research

programs and individual-investigator and small-group activities should be undertaken.

The panel recommends that the National Science Foundation increase its support for individual principal investigators conducting university-scale programs in basic research because this is most closely associated with NSF's mission. Increased support for basic research by the Department of Energy is recommended because DOE is charged with responsibility for both the magnetic and inertial confinement fusion programs, as well as a number of other energy-relevant programs that are critically dependent on the fundamental principles of modern plasma science.

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Theoretical and Computational Plasma Physics

INTRODUCTION AND BACKGROUND

Plasma physics is the study of collective processes in many-body charged-particle systems. Like the fields of condensed matter physics and molecular biology, plasma physics is founded on well-known principles at the microscopic level. In the case of plasma physics, the description is based on the Liouville equation or kinetic equations for the electron and ion distribution functions in a multidimensional phase space and Maxwell's equations, whose sources are self-consistent moments of the distribution functions. The plasma state is distinguished by the existence of a vast number of collective motions over a very wide range of spatial and temporal scales. The interaction of these collective motions often leads to turbulence or coherent patterns and structures. Indeed, coherent patterns frequently may coexist with turbulence. A priori theoretical prediction of plasma behavior has enjoyed only limited success. Therefore, experiments are critical to the identification of fundamental processes in a plasma, such as the evolution of coherent structures arising from nonlinear interactions. These, in turn, form the intellectual building blocks for understanding the evolution of yet more complex processes.

The history of plasma science is as diverse as the subject itself. In Chapter 8 above, early work in laboratory plasma science is described, beginning with the work of Faraday in the 1830s on the chemical transformation of the elements and continuing with Langmuir's work on gas discharges in the 1920s and research on electron beams and beam-type microwave devices in the 1940s and 1950s. Within a decade of Langmuir's work, the discovery that radio waves reflect from the ionosphere established the existence of the space plasma that surrounds the Earth. A new era in plasma physics began with the international development of efforts to achieve controlled thermonuclear fusion in the 1950s and with the space program, which began with the launching of Sputnik in 1957. For the past 30 years, space, fusion, and the development of advanced weapons systems have been the main drivers for plasma science.

Early in the space and fusion programs, a rich variety of fundamental configurations and phenomena were investigated, but as a rule, nonlinear processes—although fascinating scientifically—proved to be a detriment to the achievement of fusion plasma conditions in the laboratory. As a consequence, fusion research evolved to focus on systems with the least complexity consistent with programmatic goals. Inertial fusion research evolved in directions that either minimized nonlinear laser-plasma interactions or optimized particle-beam drivers. Magnetic fusion research concentrated on the tokamak approach, the most stable axisymmetric confinement configuration. The principal difficulty encountered in fusion and in defense applications has been the inability to predict the nonlinear behavior of plasmas to an accuracy required by engineering considerations. A successful example of such a prediction is illustrated in Figure 9.1.

In the exploration of space plasmas, it was not possible to reduce the natural complexity of the magnetic field geometry through engineering design. Spacecraft data have identified many key nonlinear phenomena: collisionless shocks, bursty and steady magnetic reconnection, double layers, current sheets, dynamo generation of magnetic fields, and the overall structure of magnetospheric plasmas, which are high-mirror-ratio magnetic confinement configurations. Up until now, because spacecraft obtain local data, only the rudimentary aspects of these processes have been measured.

While the discoveries of plasma phenomena in the space environment are remarkably varied, their abstractions into basic plasma processes subject to investigation by computational simulation, laboratory experiments, and analytical theory have lagged because support, especially for laboratory experimentation, has "practically vanished" in the words of the Brinkman report, *Physics Through the 1990s*.[1] Notable exceptions exist, of course, and these are presented later in this chapter.

The next decade could promise a fundamental reversal of this paradigm, provided the resources for basic plasma experimentation described in Chapter 8 become available. One can anticipate that plasma phenomena discovered through spacecraft and astronomical observations, as well as fusion research, will play an important role in motivating laboratory experimentation. Moreover, the theo-

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[1] National Research Council, *Plasmas and Fluids*, in the series *Physics Through the 1990s*, National Academy Press, Washington, D.C., 1986.

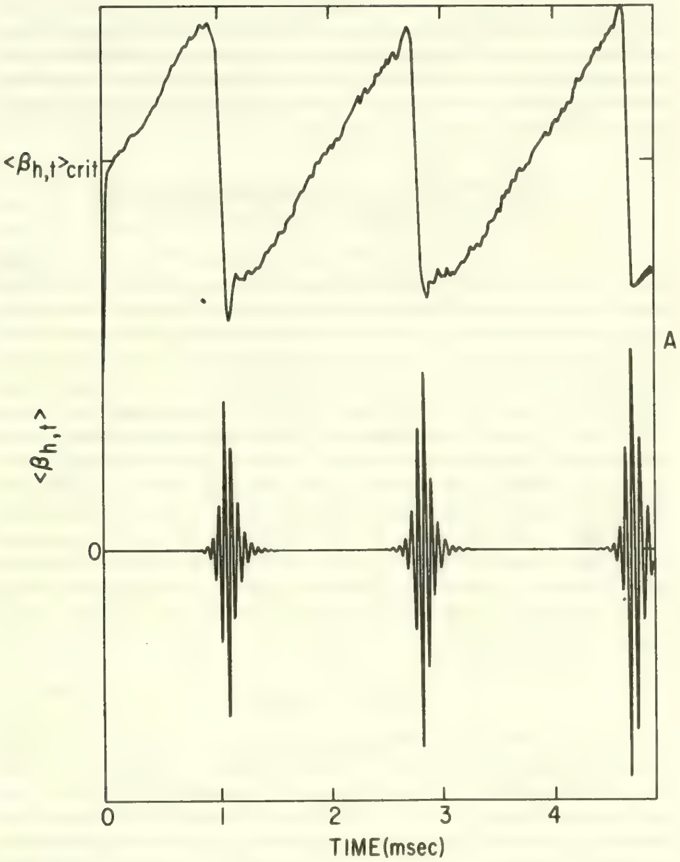

FIGURE 9.1 Theoretical model of the "fishbone" oscillations observed in tokamak plasmas. This figure shows that the high-frequency modulation of the magnetic field occurs in bursts (lower trace); it also shows the induced loss of high-energy particles during each burst, by the decrease in the normalized pressure of the energetic particles, β_h. (Reprinted, by permission, from L. Chen, R.B. White, and M.N. Rosenbluth, Physical Review Letters 52:1122, 1984. Copyright © 1984 by the American Physical Society.)

retical and simulation capabilities developed to understand this new generation of small- to intermediate-scale laboratory experiments will set standards for modeling space and astrophysical plasmas. Technological advances promise to create fundamentally new classes of plasma experiments and to enable new diagnostics. For example, as discussed in Chapter 8, our conceptual understanding of plasma dynamics will be enriched by visualization techniques only now becoming available for plasmas.

RECENT ADVANCES IN THEORETICAL AND COMPUTATIONAL PLASMA PHYSICS

Fusion, space exploration, and defense applications have been the engines of high national priority that have powered fundamental advances in plasma theory and computational plasma physics. In turn, the improved understanding of basic plasma processes has led to the seminal development of important new concepts and applications. Without attempting a complete delineation of significant achievements, in this section the panel highlights selected advances in analytical and computational plasma physics during the past decade that have resulted from the interaction of plasma theory with laboratory experiments and, to some extent, with space and astrophysical plasma measurements, because similar physics manifests itself in plasma systems of vastly different physical scales. At present, laboratory experimentation is dominated by research on magnetic and inertial fusion. Smaller experimental efforts can be found in active space experiments, nonneutral plasmas, coherent radiation generation, advanced accelerator concepts, and turbulent Q-machine plasmas. Additional advances in plasma theory and computations are incorporated in the chapters of Part II covering specific plasma topics.

Hamiltonian Transport

Apparently dissipative processes, such as particle diffusion, can occur in conservative Hamiltonian systems whenever chaos is present. In the 1980s, advances in understanding such transport were driven largely by anomalies observed in hot, effectively collisionless, magnetically confined plasmas, in which both the particle orbits and the magnetic field line trajectories obey Hamiltonian equations. A plausible contribution to anomalous loss is the effective diffusion induced by such chaotic behavior. Recent numerical and analytical studies have shown that Hamiltonian transport rates can depend sensitively on such unexpected structures as turnstiles, devil's staircases, and stochastic webs. Moreover, the ideas have been applied to estimating the loss of energetic charged particles from magnetically confined systems, reducing the necessity for elaborate and expensive numerical calculations based on guiding-center theory.

Coherent Structures and Self-Organization

The theoretical discovery of solitons, long-lived coherent solutions to certain nonlinear fluid equations, arose out of plasma physics research in the 1950s. In recent years, a more general class of nonlinear structures, including both solitons and less permanent, but still robust objects (solitary waves), has been found to play a significant role in plasma evolution. Thus, large-scale turbulence is often dominated by vortical structures, analogous to fluid vortices, but depending on the interaction of the plasma with electromagnetic fields. At smaller scales, such phase-space structures as clumps or holes can critically affect plasma dissipation. The past 10 years have seen significant progress in classifying, explaining, and assessing the importance of such phenomena. Their importance in laboratory plasma confinement, nonneutral plasmas, magnetosphere evolution, and solar physics is now firmly established, although much of the difficult nonlinear physics remains to be understood.

Strong Plasma Turbulence

The past decade has contributed to a greater, although still incomplete, understanding of plasma turbulence. Strong turbulence theory has been applied to many microinstabilities (drift waves and ion temperature gradient, trapped-particle, microtearing, and magnetic modes in tokamak plasmas). Resonance broadening has been addressed, and the direct interaction approximation (DIA), although still heuristic and difficult to implement numerically, has been extended to provide a general form that can be used for simpler transport models. Some understanding has been developed of turbulent cascades in plasmas, of nonlinear transport mechanisms, and of the coupling of heat and particle transport. Numerical studies of solar convection have greatly improved our understanding of turbulence in stars.

Gyrokinetics

During the past decade there has been a refinement of gyrokinetics, the approximate theory of the motion of charged particles in strong magnetic fields, with applications to stability theory and magnetohydrodynamics. Of particular note is the successful application of this description to the numerical simulation of a class of slow instabilities, the ion temperature gradient mode, resulting in mode spectra in excellent agreement with tokamak experiments. Also, a hybrid magnetohydrodynamic-gyrokinetic code has successfully simulated both fishbone and toroidal Alfvén eigenmodes, although greater computing power is needed to definitively study the latter.

Large-Orbit Effects on Plasma Stability

The influence of large-orbit particles in a plasma on low-frequency stability was computed by the Vlasov formalism in the form of a modified energy principle. The observed stability of field-reversed configurations has been attributed to this effect. A formal theory of interaction of a dilute species of energetic particles (described by the Vlasov equation) with magnetohydrodynamic Alfvén waves (described by fluid equations) has been developed and applied, with quantitative success, to tokamak plasmas and to the magnetosphere. The complex geometry of tokamaks, which is periodic the short-way-around the doughnut, altered the Alfvén wave propagation and attenuation bands as periodic media generally do. Almost-undamped Alfvén wave modes emerged that could be destabilized by energetic particles with velocities comparable to the Alfvén speed. This development forms the basis on which to expect challenging physics when thermonuclear reactions take place in magnetically confined plasmas.

Three-Dimensional Magnetohydrodynamics

Three-dimensional resistive magnetohydrodynamic simulations have successfully modeled turbulent generation of toroidal flux in force-free reversed-field pinch experiments. Three-dimensional resistive magnetohydrodynamics further gives a good account of magnetic reconnection in tokamaks and associated magnetic oscillations, including spontaneous formation of singular current sheets. However, a few troubling enigmas remain to be explored.

Numerical Simulation of Plasma Processes

The numerical study of plasmas has advanced markedly during the past decade, with applications to the ionosphere, the magnetosphere, solar flares, solar pulsations, stellar convection, nonlinear magnetohydrodynamics, gyrokinetics, and so on. (See Plate 8.) The progress has been due to a combination of improvements in algorithms and the advent of cheaper more powerful computers, both supercomputers and workstations, that provide great power, rapid turnaround, and networking at very modest cost. The computational discovery of nonlinear coherences that compensate for linear damping of microinstability modes in tokamaks calls into question the use of quasilinear correlation functions to estimate transport consequences of microinstabilities in tokamaks.

Nonlinear Laser-Plasma Interaction

Virtually all of the many instabilities driven by intense electromagnetic waves interacting with plasma were identified theoretically and studied in laser-plasma experiments during the past decade. Key nonlinear signatures predicted by numerical simulations and theory, such as the production of very energetic electrons, were confirmed. Various control techniques were also demonstrated, including collisional suppression and laser beam incoherence. Progress in this area had a major impact on research in inertial fusion, leading to the use of shorter-wavelength lasers.

Nonlinear Processes in Ionospheric Plasmas

The interaction of high-power radio-frequency waves with plasmas, in particular the ionosphere, has stimulated the theoretical development of a coupled ion acoustic-Langmuir wave turbulence model, generically known as the Zakharov equations. Computational studies of this model have identified spontaneous creation of cavitons—small-scale density structures that self-consistently trap Langmuir waves. Recently, fluid representations of collisionless damping have become available that will further increase the sophistication of the Zakharov approach.

Barium cloud releases in the ionosphere stimulated development of a new form of two-dimensional turbulence with key differences from two-dimensional hydrodynamic turbulence. Simulations based on these equations exhibited striking similarities to experimental releases. The equations were further applied to naturally occurring striations in the equatorial F-region and again enjoyed quantitative successes, especially with regard to the spectrum of turbulence.

The cross-magnetic-field current of the equatorial electrojet drives $\boldsymbol{E} \times \boldsymbol{B}$ turbulence in the equatorial region of the ionosphere. The nature of this low-frequency turbulence has been studied by radar backscatter diagnostics and in situ rocket campaigns. The plasma is weakly ionized so that the basic equations are well formulated and robust. The cascade theory of turbulent eddies, in the direct interaction approximation, predicts the nature of the nonlinear interactions and the line-width of the frequency spectrum, and is in accord with observations and numerical computations.

Collisional Relaxation of Nonneutral Plasmas

The consequences of binary collisions in nonneutral plasmas have been predicted to depend dramatically on magnetic field strength. In particular, when the duration of a collision, based on the distance of closest approach, exceeds the cyclotron period, the magnetic moment becomes an adiabatic invariant and the relaxation of perpendicular velocities becomes exponentially small. Quantitative experimental confirmation of an exponentially small equipartition rate between parallel and perpendicular temperatures has been demonstrated.

Free-Electron Lasers and High-Power Microwave Sources

Significant progress has been made in the fundamental nonlinear theory of high-power, coherent radiation generation in free-electron devices such as gyrotrons and free-electron lasers, particularly in areas related to nonlinear saturation mechanisms and efficiency optimization, mode selection and phase stability, and the effects of stochastic particle orbits. This has led to a new generation of laboratory microwave sources that have applications ranging from heating and noninductive current drive in fusion plasmas to communications and radar.

RESEARCH OPPORTUNITIES

This section identifies future research opportunities of fundamental importance in theoretical and computational plasma physics, including basic plasma theory and applications to laboratory plasmas, and space and astrophysical plasmas. Additional research opportunities in plasma theory and computations are incorporated in other chapters of this report.

Basic Plasma Theory and Applications to Laboratory Plasmas

The creative interplay between theoretical and computational studies and laboratory experimentation has been the classic engine that advances scientific understanding. In recent years, computations have begun to serve partially the role of experiments, with "simulation experiments" revealing unanticipated structures and coherences. The panel's vision of research frontiers in plasma physics during the next decade presumes that a program in basic laboratory experimentation will come into being, so that the range of topics investigated will be appreciably broader than those topics tied almost exclusively to fusion physics and defense applications. Of course, this will serve to extend and test current theoretical capabilities and to present qualitatively new challenges. Hence, the synergism among basic plasma theory, laboratory experiments, and space and astrophysical plasma observations promises to play an appreciably stronger role during the next decade.

Continuing progress is expected to be made in all of the areas identified earlier in the preceding section, "Recent Advances." In addition, the following topics, surely not comprehensive or mutually exclusive, represent research opportunities of high intellectual challenge in basic plasma theory and applications to laboratory plasmas.

Nonlinear Plasma Processes

Much of the progress in plasma physics during the past has been made by linear (small-signal) theory, which has provided conceptual and in many cases quantitative understanding. But nonlinear theory is of great intrinsic interest and is essential for the description of most important applications of plasma physics that involve magnetohydrodynamics, kinetic theory, turbulence, the interaction of charged particles with intense electromagnetic fields, and so on. Therefore, increased attention should be given to nonlinear theory aimed at the development of new analytical and numerical tools.

Numerical Simulation

A most promising area for the future is that of numerical simulation, driven by continuing dramatic advances in computational speed and computer organization, and decreases in the cost of hardware. These ongoing improvements in hardware, coupled with the parallel design of new and more efficient algorithms, should allow the solution of many of the nonlinear problems that currently defy direct analytical solution. Numerical computation offers the best hope of dealing meaningfully with the large problems in complex geometries that characterize so many of the significant applications of plasma physics. One anticipates the development of teams of computational specialists, theorists, experimentalists, and engineers, organized to optimize the solution of particular large technical problems. Training of students for this type of operation should be encouraged in universities.

Novel Analytical Techniques

The challenge of nonlinear theory suggests the adaptation or innovation of novel analytical techniques. The use of percolation theory for certain transport problems in plasmas appears to be promising. The development of modern statistical analyses, perhaps employing artificial intelligence (symbolic dynamics), may lead to greatly improved data analysis and new physical insights. The transfer from pure mathematics of well-developed areas such as wavelet theory, which are relatively unknown in physics and engineering, offers great promise.

Boundary Layers

Boundary layers are of great importance in plasmas. These occur in such diverse applications as the sheath region near the first wall of a fusion reactor, the region in a coronal hole where the solar wind is emitted as the system changes from collision-dominated to collision-free, and magnetic reconnection in plasmas of interest in space. They are often distinguished by the need for a full kinetic theory, and they will require a synthesis of analytical boundary layer techniques and advanced numerical methods.

Kinetic Theory

In the past, much theoretical work has been done employing fluid theories in circumstances where they are strictly not valid, because fluid descriptions are more tractable than kinetic ones. Such treatments are commonplace in astrophysics, space plasma physics, and long mean-free-path fusion physics. Advances driven by novel analytical approaches coupled with advances in computation are highly likely. The systematic exploitation of dimensionless small parameters to obtain reduced kinetic descriptions is one promising approach.

Stochastic Effects in Evolving Plasmas

In plasma physics, the conceptual advances of nonlinear dynamics and stochasticity theory have been applied to abstracted, simplified problems, capable of formulation in terms of Hamiltonian dynamics. Examples include criteria for stochastic magnetic field line "orbits" in tokamak and stellarator devices, nonlinear wave-particle interactions, and particle orbits near magnetic field nulls in the magnetosphere. During the next decade a key challenge for theoreticians will be to incorporate stochasticity self-consistently into evolving plasma systems. Magnetospheric magnetic reconnection serves as a useful example. How will stochastic orbits alter the evolution of magnetic fields and field nulls when the extent of the stochastic regions must be self-consistently determined in terms of the evolving field? Diagnostic and data processing advances that can identify stochasticity in plasma systems must be developed concurrently.

Alpha-Particle Effects in Magnetically Confined Plasmas

The level of sophistication of theoretical models and instrumentation techniques, and the high quality of plasma conditions achieved in laboratory experiments have progressed to the point that the deuterium-tritium experiments planned on the Tokamak Fusion Test Reactor (TFTR) and the Joint European Torus (JET) are expected to elucidate important physics issues regarding the influence of alpha particles on plasma stability and transport processes. There are two major phenomena associated with the appearance of significant densities of alpha particles in fusion devices. First, coherent plasma oscillations can be excited by resonant interaction with the alpha particles, either at low frequencies, corresponding to the toroidal precession rate, or at high frequencies, corresponding to the direct interaction of alpha particles with shear Alfvén waves. Such collective oscillations are driven unstable by the alpha-particle pressure gradient and result in the rapid expulsion of a significant number of high-energy particles. Several such modes have already been predicted theoretically and identified on existing devices, where they are excited by high-energy particles produced by injected beams or ion cyclotron heating. Second, high-energy particle orbits are modified by the presence of toroidal field ripple, and if sufficiently large, ripple can cause the orbits of trapped particles to become stochastic and to be lost from the plasma in a time that is short compared to the slowing-down time.

Concept Improvement

As the tokamak confinement approach continues to make significant technical progress toward achieving the conditions required for fusion power production, it is increasingly important to improve the tokamak concept, particularly in the long-pulse and steady-state regimes that can lead to compact designs for economical power production. This will require a significant theoretical and experimental effort in the study of advanced-tokamak regimes that optimize the bootstrap-current fraction produced by the collisional equilibration between trapped and passing particles, improve the efficiency of noninductive current drive, optimize the current profile and plasma shapes, and explore regimes of enhanced confinement and increased plasma beta. To ensure fully equilibrium (i.e., "steady-state") conditions, these plasma regimes must be studied for pulse lengths longer than the characteristic time scales of plasma processes and plasma-wall interactions. This will require steady-state power handling, particle exhaust, and impurity control by divertors at high power fluxes, as well as the development of advanced plasma fueling, current-drive, and control techniques. These are key features of the Tokamak Physics Experiment (TPX) planned for operation around the turn of the century.

Nonlinear Interaction of Intense Electromagnetic Waves with Plasmas

During the next decade, more quantitative models must be developed that describe the nonlinear interaction and competition of plasma instabilities driven by intense electromagnetic waves. This is a timely challenge for many reasons. In laser-plasma experiments, these instabilities are being characterized in greater detail, using increasingly sophisticated diagnostics. Nonlinear models based on the Zakharov equations are already illustrating the rich competition between different instabilities, and successful comparisons with experiments are being made. Finally, advances in computational physics and computers are allowing improved simulations, including, for example, meaningful three-dimensional simulations of laser-beam filamentation by both ponderomotive and thermal mechanisms.

More quantitative models of the nonlinear behavior of laser-plasma instabilities would allow optimized regimes of operation for inertial fusion and improved interpretation of data from ionospheric and space plasmas. Because these instabilities involve the most basic plasma waves, improved understanding of the nonlinear behavior would be a very significant contribution to plasma science, no doubt stimulating new advances and applications.

Current-Carrying Plasmas with Flow

One research opportunity in laboratory plasma physics, which is synergistic with space and astrophysical plasmas (see next section, "Space Plasmas"), is the investigation of current-carrying plasmas with flow. In these plasmas, global and/or turbulent flows are essential to the physics. By contrast, magnetic fusion plasmas are effectively stationary. Flowing plasmas present a new challenge to the experimentalist to design facilities in which the desired phenomena occur and to develop scaling arguments that laboratory plasmas are representative of the physics of space and astrophysical systems. To the theorist and applied mathematician, flowing plasmas are no less of a challenge because of the presence of several varieties of discontinuities, which must be understood as isolated, often collisionless processes and then incorporated self-consistently into the overall model just as hydrodynamicists incorporate shocks into supersonic flows. Discontinuities abound in plasmas. Even flows with velocities well below the Alfvén speed lead to singular current sheets in tokamaks and in the solar corona. And one need look no further than the solar photospheric magnetic field, which is concentrated into small regions of high intensity, to recognize that any theoretical understanding of dynamo generation of magnetic fields must accommodate an extraordinary degree of spatial intermittency. There has yet to be a successful laboratory demonstration of a hydromagnetic dynamo. With adequate support, visualization diagnostics can be implemented that promise to yield insights into magnetohydrodynamic flows comparable to those observed in hydrodynamic experiments.

Engineering Design Tools

The next decade should also see the development from the results of well-founded theories of simpler but robust tools for engineering design in the several areas of application of plasma physics, such as magnetic and inertial fusion, microwave devices, high-efficiency lamps, plasma processing, and particle accelerators.

Space Plasmas

In 1978, the National Research Council report *Space Plasma Physics: The Study of Solar System Plasmas*, prepared by a Space Science Board study committee headed by Stirling A. Colgate, strongly endorsed space plasma physics as "intrinsically an important branch of physics."[2] The Colgate report is widely considered to be the impetus for the present programmatic emphasis in the field.

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[2] National Research Council, Space Science Board, *The Study of Solar System Plasmas*, National Academy Press, Washington, D.C., 1978.

Specifically, it identified six areas of research as important to develop the basic understanding of space plasmas and, therefore, of fundamental intellectual value to plasma physics. The areas are magnetic reconnection, turbulence, the behavior of large-scale flows, particle acceleration, plasma confinement and transport, and collisionless shocks. Substantial progress has been made in each area over the intervening 14 years. Examples are cited here and in Chapter 6, which is devoted specifically to space plasma physics. All of these areas offer significant research opportunities in theoretical and computational plasma physics during the next decade. Future research opportunities in space plasma theory are summarized below.

Magnetic Reconnection

Magnetic reconnection is a process by which stored magnetic energy can be converted explosively into plasma kinetic energy. It is invoked ubiquitously as a mechanism in space and astrophysical problems—in solar and stellar flares, which occur commonly in stressed bipolar magnetic regions; in flux transfer events, which intermittently erode the outer layer of Earth's day-side magnetic field and add the flux to a stressed antisunward magnetotail; and in the magnetospheric substorm, where the calamitous relaxation of the magnetotail is accompanied by pronounced auroral brightenings, enhanced electrical currents in the ionosphere, and the antisunward escape of a large blob of accelerated plasma. Observation of the reconnection process itself is fragmentary, and evidence is circumstantial. The mechanism occurs readily in models based on the magnetohydrodynamic (MHD) equations but is due to resistivity, externally postulated or spuriously generated by numerical discretization. Space plasmas are collisionless to a high degree, and reconnection is significant only when the resistivity due to classical two-body interactions is enhanced by anomalous collective processes. Candidate mechanisms such as the lower-hybrid-drift instability have been suggested. Further research needs to be carried out to investigate the microphysics and integrate the results into an MHD description of large-scale behavior. In turn, quantitative MHD predictions must benefit from the enhanced numerical capabilities of new computer architectures and algorithms that maximize their effectiveness.

Turbulence

Space plasmas are characterized by turbulence on all scale lengths. Hydrodynamic turbulence occurs in the convection zone of the Sun and, through coupling to rotation, may play an important role in the dynamo mechanism and global oscillation modes. Models that explore these processes on an elementary scale are being developed but are limited by numerical considerations. Magnetohydrodynamic turbulence is generated in the solar wind by the intersection of plasma streams emanating from different longitudes on the rotating Sun. This turbulence plays an important role in heating the solar wind. Indeed, the solar wind has become a fundamental medium for investigating MHD turbulence—its generation, evolution, and dissipation—with advanced statistical concepts. An unusual, microscale turbulence occurs when newly created plasma experiences the solar wind blowing across it, as is the case when cometary molecules are ionized. This turbulence is important in transferring momentum from the solar wind and can produce a deflection of the solar wind flow if the so-called mass loading is significant. Microturbulence plays many roles in magnetospheric plasma physics. Its importance in the magnetic reconnection process has already been mentioned. Another example, especially well studied from the standpoint of observations, modeling, and theory, is equatorial spread-F, a fluid-like turbulence in which flux tubes interchange in the low-latitude regions of Earth's collisional ionosphere.

Large-Scale Flows

The outward flow of the solar wind is one of the permanent features of space plasmas. Its speed varies by factors of two or three temporally and exhibits marked spatial variations, but the phenomenon is seen in every possible observation. Much less documented and understood is the coupling of such flow to planetary magnetospheres. Low-altitude observations at Earth indicate that the magnetospheric plasma is impelled into convection by coupling to the solar wind. The processes for this coupling are poorly understood: a steady magnetic reconnection may be the agent, or the magnetopause, the outer magnetospheric boundary, may experience Kelvin-Helmholtz instability so that the flow penetrates viscously. However, there is unanimity that the controlling processes take place in spatially localized boundary layers, with dimensions of the order of a few ion Larmor radii. The sense of the flow is antisunward at high latitudes and sunward at low latitudes, so that a circulation pattern is established. Because of the high electrical conductivity along magnetic flux tubes, it is expected that the entire magnetospheric plasma must participate in this convection. However, years of effort to measure steady magnetospheric convection far from Earth's surface largely have been inconclusive. The most recent data analysis suggests that the process may proceed in a bursty fashion. If so, large constraints would seemingly exist on the underlying physics: that it be initiated locally and hence probably involve microprocesses, and yet that it be transmitted globally on a rapid time scale.

Particle Acceleration

Because of their strong manifestations, energetic charged particles are central to astrophysics, and effort has focused there on acceleration processes that elevate their energy selectively from a thermal bath. Space plasmas are an accessible microcosm for examining some acceleration processes. Indeed, the observational origins of space plasma physics trace to cosmic-ray physics. Acceleration processes can be either systematic or random. An example of the former is the function of electric double layers that form at low altitudes along terrestrial magnetic field lines as a result of collective processes. Double layers have been created in laboratory devices and replicated in numerical simulations, but owing to their spatial extent, like so many phenomena they are difficult to identify unambiguously from space data. Much more widely invoked, because it is a process capable of raising particles to very high energies, is stochastic acceleration by random and repeated encounters with electromagnetic fluctuations. Outstanding success has been achieved in analyzing this mechanism in connection with shock-induced fluctuations. The treatment has been kinetic and self-consistent in the sense that the accelerated particles contribute to the energizing wave spectrum. An important next step is to assess the relative importance of such accelerated particles to the shock structure itself.

Plasma Confinement and Transport

Plasma confinement and transport are widely inclusive concepts. At the time of the Colgate study,[3] the primary reference was to the longevity of energetic, magnetically trapped particles, such as those populating the Earth's Van Allen belts. Like the solar wind, these are enduring features of the Earth's space environment, and analogous structures have been discovered in the environs of all the magnetized planets. The slow loss of Van Allen particles to Earth's atmosphere due to Coulomb collisions at very low altitudes and wave-particle scattering at higher altitudes is well understood. Of a much more speculative nature is the process that replenishes the belts so that they maintain their long-term existence. The issue is especially important at Jupiter, where MeV electrons are continuously losing energy due to synchrotron radiation. Present conjecture is that global-scale, low-frequency electromagnetic fluctuations, perhaps induced by solar wind buffeting of the magnetosphere or time-dependent atmospheric dynamo processes, allow particles to randomly traverse magnetic field lines to close-in distances, gaining energy in the process. Because of the global nature of the physics, it is difficult to verify this process observationally. What has been done is to create diffusion models using representative fluctuation spectra and to compare output particle distributions with observations—and this has been carried out with reasonable success. However, confinement and transport issues during the next decade will undoubtedly be far more expansive. To what extent are the outer reaches of the Earth's magnetosphere and the Sun's atmosphere equilibrium structures? If the current direction of thinking is correct and gross plasma behavior is regulated by processes at a number of thin boundary layers, such as stream-interaction regions in the solar wind and Earth's plasma sheet boundary layer, what processes are responsible for maintaining the existence of such narrow structures?

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[3] See footnote 2, p. 167.

Collisionless Shocks

The study of collisionless shocks is arguably the area in which the most significant advances have been achieved during the past decade and where the impact of space on basic plasma physics has been most profound. Success has been achieved as the result of a coordinated approach to the problem, using sets of complementary observations analyzed in the context of contemporary numerical models that integrate relevant microscopic theory. The Earth's bow shock is observable on every spacecraft orbit that reaches the solar wind. The International Sun-Earth Explorer (ISEE) mission is a cooperative venture between NASA and the European Space Agency (ESA). ISEE, with its coordinated pair of maneuverable spacecraft having ideally suited apogees, featured bow-shock physics as a prime scientific objective and has contributed immeasurably to this understanding. Bow shocks have been observed in association with every planet and comet. They are necessary hydromagnetic structures allowing the supersonic, super-Alfvénic solar wind to slow down and divert around an obstacle in its flow. Collisionless shocks are also common coronal and solar wind phenomena. There is rich variety to shocks, and this diversity is still incompletely investigated and understood. A prime determinant is the direction of the magnetic field in the incoming flow with respect to the normal to the discontinuity surface. If the angle is large, the shock is a perpendicular shock and generally laminar and quiescent. If the angle is small, the shock is a parallel one and exhibits a great deal of pulsation, structural disintegration, and reforming. Owing to the geometry, different azimuthal sectors of the same planetary bow shock can be quasiperpendicular, while other sectors are quasiparallel. In both instances there is a wealth of fine-scale plasma structure, both internal and external to the discontinuity.

Chaotic Effects

During the past decade, realization of the importance of chaos has emerged in all branches of science. This is true of space plasma physics. The focus of attention has been on chaotic particle trajectories, especially those occurring in the equatorial region of the magnetotail where the magnetic field is significantly weakened owing to distention. It is known that a mixture of chaotic and regular orbits exists, but the relative magnitude of their numbers is an open question. First results indicate that injection at the proper locations and ensuing chaos can lead to the formation of charged-particle beams, which are commonly observed. Particles on chaotic orbits also contribute significantly to the self-consistent current density responsible for the magnetic distention. If chaotic orbits are important, they must also certainly impact collective kinetic phenomena. Study of this subject is still in its infancy. It is quite plausible that similar physics takes place in the other boundary layers mentioned earlier in this section.

Summary

To summarize, space plasma physics has reached a mature phase. Space plasma physics has passed through the exploratory phase and is beginning an era of understanding. Future efforts will focus on the details of specific structures and processes. Theory and modeling at all levels have become a routine element of missions. As the morphology of and scientific basis for structures and processes become clearer, it is important that a basic understanding of the underlying plasma physics be pursued directly by relevant laboratory simulations, to test quantitatively and in detail the predictions of new theories as they become available.

CONCLUSIONS AND RECOMMENDATIONS

Plasma physics is the study of collective processes in many-body charged particle systems. The state of such a system departs considerably from the strictly thermodynamic equilibrium state. The understanding of collective processes is the central goal of theoretical plasma physics. There have been significant advances in theoretical and computational plasma physics during the past decade. Priority should be assigned to the research opportunities of high intellectual challenge identified earlier in this chapter. In the remainder of this section, the panel presents a succinct summary of principal findings and recommendations.

Fusion, space exploration, and defense applications have been the engines of high national priority that have powered fundamental advances in plasma theory and computational plasma physics. In turn, the improved understanding of basic plasma processes has led to the seminal development of important new concepts and applications.

The panel recommends that a vigorous program in plasma theory and modeling efforts in fusion and space exploration should be continued. This is essential for continued progress in the interpretation of experiments and the development of new concepts in these important national programs.

Support for individual university investigators to explore fundamental plasma theory and innovative concepts is at precariously low levels. This situation threatens the continued development and nurturing of the very intellectual foundations of modern plasma theory.

The panel recommends that the program of individual-investigator research in basic plasma theory should be reinvigorated to explore the broad range of intellectually challenging problems in stochastic effects, novel analytical techniques, nonlinear processes, and other areas, which are essential to the continued vitality of plasma theory as a scientific discipline.

Plasma theory is sufficiently advanced that a predictive capability exists for describing the properties of many static plasma configurations and simple wave-particle interactions. However, flowing, turbulent, and highly nonlinear saturation processes are at the forefront of analytical and computational capabilities.

The panel recommends that the design of experiments jointly by theoreticians and experimentalists to elucidate the conceptual foundations of nonlinear plasma physics should be encouraged.

The panel recommends that plasma theory should be encouraged that seeks to establish a commonality of physical processes and applied mathematical techniques across a wide range of realizations, from pellet compression in inertial fusion to plasma processes on astrophysical scales.

Advances in nonlinear plasma science in the past have relied heavily on the insights gained from numerical simulation. The panel envisions that future advances in theoretical plasma physics will have even greater reliance on numerical techniques and on the increased computational capability and visualization techniques available in present-day and future computer systems. A particular challenge is posed by discontinuities such as shocks, current sheets, and double layers.

The panel recommends that emphasis should be placed on ongoing programs in grand-challenge computations. Emphasis also should be placed on plasma computations investigating processes common to a wide range of scales.

The following of the panel's general recommendations (see Executive Summary) are made to improve the national effort in theoretical and computational plasma science:

1. To reinvigorate basic plasma science in the most efficient and cost-effective way, emphasis should be placed on university-scale research programs.
2. To ensure the continued availability of the basic knowledge that is needed for the development of applications, the National Science Foundation should provide increased support for basic plasma science.
3. Individual-investigator and small-group research, including theory and modeling as well as experiments, needs special help, and small amounts of funding could be life-saving. Funding for these activities should come from existing programs that depend on plasma science. A reassessment of the relative allocation of funds between larger, focused research programs and individual-investigator and small-group activities should be undertaken.

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Education in Plasma Science

For plasma physics to make the contributions in the areas identified elsewhere in this report, there must be enough researchers and applied scientists knowledgeable in the plasma fields to enable those contributions to be made. To examine the demographics of the plasma physics field, data were obtained from the American Institute of Physics (AIP), the National Science Foundation, the National Research Council (NRC), and a survey of doctorate-granting universities.

The data indicate how many scientists were educated at the doctoral level in plasma physics. These scientists are not all of those now working in the field. The survey information also indicates how many new doctoral-level researchers will be entering the job market in the next five years. These, along with the current practitioners, must meet the challenges for plasma physics identified in the rest of this report.

DEGREE PRODUCTION AND EMPLOYMENT STATISTICS

From 1965 to 1991, 1539 doctorates were awarded in plasma physics. The average annual production in the 1970s was 72, with an upsurge between 1970 and 1972, peaking at 93 PhDs in 1972. The average annual production in the 1980s was 55. The number dropped to 42 in 1990 and rose to 58 in 1991.[1]

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[1] National Science Foundation, *Science and Engineering Doctorates: 1960-1991*, NSF 93-301, Washington, D.C., 1993, Table 1.

Graduates in plasma physics have been primarily white male U.S. citizens: of the 1960-1991 total, nearly 97% were male and 74% were U.S. citizens. Of the 1976-1991 plasma physics doctorate recipients, 65% were white. In 1991-1992 there was a change in the nongender categories: plasma physics PhD recipients were 61% U.S. citizen and 53% white, but still 96% male.

The AIP provided data on employment based on a sample estimate of approximately one-tenth of all plasma physicists who are members of the AIP.[2] This information indicates that for PhD AIP members working full or part time in plasma physics in 1990, there is not one predominant category of employer. Four national laboratories, the university sector, and industry each account for about one-third of the positions:

University or university-affiliated research institute	34%
Federally Funded Research and Development Center (FFRDC)	34%
Industry	23%
Government	8%
Self-employed	1%

Most employees of FFRDCs were at Lawrence Livermore National Laboratory (LLNL), Princeton Plasma Physics Laboratory (PPPL), Los Alamos National Laboratory (LANL), and Naval Research Laboratory (NRL).

Plasma scientists associated with universities are often on the research staff of the university, not the teaching faculty:

Research staff (e.g., research scientist)	47%
Professor	33%
Associate professor	6%
Assistant professor	6%
Other/unknown	8%

Although the data do not indicate tenure-track versus non-tenure-track positions, the predominance of research staff positions suggests strongly that a large number of plasma physicists in university-associated positions are not on the tenure track.

Similar data for other fields in 1990 are given in Table 10.1. In none of these fields do physicists appear as likely to be in a non-tenure-track position as in plasma physics. This point probably is not missed by graduate students selecting a field.

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[2] Information from AIP Statistics Division, included with letter from Jean M. Curtin, research associate, to John Ahearne, September 16, 1992.

TABLE 10.1 Employment Category in 1990 (percent of total) for University-affiliated Physicists in Selected Fields

	Nuclear Physics	Condensed Matter Physics	Atomic and Molecular Physics	Elementary Particles and Fields	Optics
Research staff	17	21	21	20	27
Professor	56	46	48	59	40
Associate professor	11	15	16	10	15
Assistant professor	9	17	11	10	17
Other/unknown	7	1	4	1	2

Source: Information from AIP Statistics Division, included with letter from Jean M. Curtin, research associate, to John Ahearne, February 9, 1993.

TABLE 10.2 Area of Employment in 1990 (percent of total) for Holders of PhDs in Selected Physics Fields

	Nuclear Physics	Condensed Matter Physics	Atomic and Molecular Physics	Elementary Particles and Fields	Optics
In field	29	45	28	40	58
Other physics	47	31	47	40	26
Engineering	7	10	9	8	6
Other/unknown	17	14	16	12	10

Source: Information from AIP Statistics Division, included with letter from Jean M. Curtin, research associate, to John Ahearne, February 9, 1993.

In 1990, about half of those who held a PhD in plasma physics were working primarily on plasmas; their subfields of employment were as follows:[3]

Plasma physics	51%
Other physics	30%
Engineering	12%
Other/unknown	7%

For comparison, Table 10.2 indicates areas of employment in 1990 for holders of PhDs in other physics fields. For all these degree fields, at least 75% of PhD recipients were in some field of physics. However, only for plasma and optics were at least one-half the doctorate holders working in the same field as their doctorate.

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[3] Information from AIP Statistics Division, included with letter from Jean M. Curtin, research associate, to John Ahearne, February 9, 1993.

Although many who were educated as plasma physicists have switched to other fields, crossover into plasma physics has also occurred. The AIP data indicate that many people working in plasma physics were educated in other fields. For those who indicated in 1990 that they were working full or part time in plasma physics, the predominant degree was as follows:[4]

Plasma physics	56%
Other physics	27%
Engineering	8%
Mathematics and statistics	1%
Unknown	8%

ESTIMATE OF FUTURE SUPPLY OF PLASMA PHYSICISTS

The NRC Doctorate Records Project identified 52 U.S. academic institutions that awarded at least one doctorate identified as in the field of plasma physics during the period from 1987 to 1991.[5] These 52 institutions awarded a total of 298 doctorates in plasma physics during this period. The data do not indicate from which department the degree was awarded.

Questionnaires were also sent to the chairs of physics departments (or other departments, if they had been identified as more appropriate). Responses were received from 40 departments, representing 38 institutions. The responding institutions produced 255 PhDs in 1987-1991 (86% of the total identified by the Doctorate Records Project). The departments estimated that during the next five years they would produce 332 to 340 PhDs in plasma physics—an increase of at least 11% over the previous five years. In the respondents' departments, in addition to 374 students in doctorate programs, there were 31 students in master's programs. Thus, if the previous supply of plasma physicists was enough to meet the needs of the field (implied by nearly one-half not working in plasma physics), unless there is a very large growth in demand the current estimated production rate should be more than adequate. It may be too high, which could be true of all physics, as indicated by the head of the AIP Education and Employment Statistics Division, Roman Czujko: "Results from AIP's most recent surveys indicate that the total number of projected vacancies in academia, government, and national laboratories combined is well under 50% of the total number of physics PhDs produced each year (approximately 1,260 in 1991). . . . There is no way that hiring in industrial settings will make up the difference completely."[6]

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[4] See footnote 3.

[5] Information included in letter from Lori Thurgood, research associate, NRC Office of Scientific and Engineering Personnel, to John Ahearne, October 13, 1992.

The fusion program is responsible for a large number of prospective plasma PhD graduates. Of the seven schools that reported that they expected to award at least 20 PhDs in plasma physics over the next five years, five have strong fusion programs, including the top four in expected graduates. These seven schools account for 57% of the total expected PhDs.

Of the 40 departments that responded to the questionnaire, 22 were physics departments. Others included space or astrophysics under various titles, applied physics, and several engineering departments. The number of "required" credit hours of course work for a plasma physics PhD averages 12.5 for the schools indicating a requirement; many recommend, but do not require, specific courses.

EDUCATING NON-PLASMA STUDENTS IN PLASMA PHYSICS

Of the 40 departments that responded to the questionnaire, 25 reported that they still have a plasma program. All 25 of these also offer courses for non-plasma science students. Usually this is a one- or two-semester course in plasma physics. Other courses offered include fusion, plasma transport, kinetic theory, and various space-related topics. The number of students taking these courses ranges from 1-2 to 20, with the usual number being 5 to 10. The bulk of these students are from physics or an engineering discipline.

GENERAL COMMENTS

Although degree production in plasma physics appears reasonably good, based on the number of expected PhDs, there are signs of erosion. Only 63% of the responding departments offer a major or a formal program in plasma physics, with 13 departments indicating they expected to award no doctorates in plasma physics in the next five years, though they had awarded a total of 27 during 1987-1991.

The following comments are typical of those provided in response to the question, If you no longer have a program to award a doctorate in plasma physics, why did the program end?

- "Lack of interest on the part of students. . . . Our one professor retired."
- "Lack of interest and lack of appropriate faculty."
- "For many years I did have a research program and funding, . . . and a number of PhD students did their theses in my lab. This research is no longer funded, and there is no one in the department now doing plasma work."

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[6] *APS News*, Vol. 2, No. 2, Feb. 1993, p. 10.

- "Lack of faculty interest and student demand."
- "Plasma physicists left or retired; research interests in our department and university changed."

As we consider arguments for strengthening basic plasma sciences in universities, the following comment from one of the respondents offers some suggestions:

> Students are trained in fundamental and advanced plasma physics, whose theses are in such diverse topics as accelerator physics, solar physics, magnetospheric physics, and ionospheric physics. These are areas which are still well-funded by NSF and NASA. . . . A degree in "pure plasma physics" is not advantageous to students these days, although the training in plasma physics of students who receive their degrees in these other areas is practically indistinguishable from the training of a student who elsewhere might get a degree in plasma physics, per se. For example, we require competence . . . in electromagnetic theory, classical mechanics, and nonlinear dynamics, quantum mechanics, kinetic theory, and fluid mechanics, as well as in the usual plasma physics topics.

Plasma physics is a foundation for many areas of physics. A similar, but broader point was made recently by Donald Langenberg, president of the American Physical Society:[7]

> I'm afraid that we have managed to convince some of our most able young students that the only thing worth doing if you're a physicist is working at Fermilab, CERN, or in a university physics department. And if you don't, you're a failure in life. It just isn't so. There are few educational and training environments that better fit a young person for a very broad array of activities than physics. The key is not to cut back on the number of physicists, but to become much more flexible in our thinking about what physicists do.

The panel believes that plasma theory would be useful to many areas of physics and has three specific suggestions: (1) There is a need for short books or early chapters in larger books that would develop "plasma literacy" for non-plasma scientists. Many graduate students would be prepared to invest time to develop a basic level of plasma knowledge, but would do so only if less than a full course were available. (2) There is a need for senior-level, undergraduate texts on plasma science. (3) For more in-depth development, texts are needed that focus on disciplines.

Astrophysics serves as an illustrative example. The standard graduate curriculum in astrophysics contains graduate physics courses, such as quantum mechanics, electrodynamics, statistical mechanics, and classical mechanics. There also are standard astrophysics courses, such as stellar structure and evolution, stellar atmospheres and radiative transfer, interstellar medium, and galaxies and cosmology. At many universities, no courses in plasma physics are taught in the physics or astrophysics departments, and although plasma courses may be given in an engineering or applied science department, these often have too technological an orientation to attract astrophysics students. Depending on the inclination of the instructor, some plasma physics may be integrated into one or more of the astrophysics courses. However, very few astrophysics students receive much formal exposure to plasma physics and many astrophysicists view it as an arcane specialty.

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[7] *APS News*, Vol. 2, No. 2, Feb. 1993, p. 7.

Nevertheless, many astrophysicists might like to learn more plasma physics when motivated to do so by developments in their subject. For example, recent measurements of magnetic field strengths in dense, star-forming interstellar clouds have shown that the fields are large enough to strongly affect or even dominate the dynamics. This has spawned a real interest in MHD among interstellar medium researchers, and a number of people who ignored magnetic fields throughout most of their careers are now writing papers on them. Such people would benefit from a good, modern text on plasma physics, stressing astrophysically interesting applications and using astrophysically relevant parameters and boundary conditions. Such a book could consist of chapters contributed by experts, provided that a good editor and refereeing system kept the quality high, and could also be used for a graduate course or seminar.

RECOMMENDATIONS

There is an increasing emphasis on industry-university partnerships, with industry moving toward greater reliance on university research. Therefore, not only is the health of university plasma science important to the academic plasma community, but it is also important for industry to have a vibrant plasma effort in universities.

The panel recommends the following steps to improve education in plasma science:

- Both industry and academic members of the plasma community should use this report to support proposals to establish tenure-track positions in plasma science. Obtaining such positions will increase the likelihood of better scientists remaining in the plasma science field, and will attract higher-quality graduate students. Particular emphasis should be placed on establishing positions in physics departments.
- Industry and academic members of the plasma community should work with faculty and administrators to provide a course in basic plasma physics at the undergraduate senior level. This would be valuable for students going on into plasmas, fusion, astrophysics, electronics, and so on.
- To better prepare scientists and engineers for the many areas in which

plasma science is important, as demonstrated in this report, plasma researchers and teachers should develop (1) texts on plasma physics focused on other disciplines (e.g., astrophysics); (2) texts specifically suited for plasma subfields (e.g., low-temperature plasmas, plasma chemistry, and plasma processing); (3) chapters on plasma science for texts in other disciplines to develop "plasma literacy" in non-plasma scientists and engineers; and (4) undergraduate, senior-level texts in plasma science.

PART IV

Conclusion

Plasma science is a diverse enterprise spanning many of the fields of science and technology. Plasmas of interest range over tens of orders of magnitude in temperature and density—from the tenuous plasmas of the ionosphere to the ultradense plasmas created in inertial confinement fusion, and from the cool, chemical plasmas used in the plasma processing of semiconductors to the thermonuclear plasmas created in magnetic confinement fusion devices. In this report, the panel has specifically assessed the diverse subfields of low-temperature plasmas, nonneutral plasmas, magnetic and inertial confinement fusion, beams, accelerators and coherent radiation sources, and space and astrophysical plasmas. Each subfield is making important contributions to our society, and much can be expected of each of these enterprises in the future.

In today's fiscally constrained environment, with major pressures to reduce federal spending, it is impractical to expect significant increases in funding. However, it is important for government decisionmakers, both in the executive branch and in Congress, to understand the value of plasma science and to recognize that some relatively minor steps can produce significant long-term gains for the United States. Plasma science is exactly the type of scientific field that a recent National Academy of Sciences report[1] stresses the United States should support:

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[1] National Academy of Sciences, *Science, Technology, and the Federal Government: National Goals for a New Era*, National Academy Press, Washington, D.C., 1993, p. 20.

> The United States should maintain clear leadership in some major areas of science. . . . among the criteria . . . are the following: . . . the field affects other areas of science disproportionately and therefore has a multiplicative effect on other scientific advances . . . .

The great diversity of the subfields and applications of plasma science poses significant challenges to ensuring the coordination of activities, the funding of research and development, and even the recognition of the contributions that plasma science makes to our society. Given this great diversity, one of the panel's central recommendations is that there is a need for increased coordination of the support for plasma-related science and technology.

The central theme of this report is that while plasma science has contributed significantly to our society and will continue to do so, it is not adequately recognized as a scientific discipline. Consequently, the basic aspects of plasma science are not adequately supported, and this is threatening the fundamental health and efficiency of our entire effort in plasma science and technology. Although the applications of plasma science are in reasonably good health, the same cannot be said for the underlying, basic plasma science. One of the central conclusions of the Brinkman report, *Physics Through the 1990s*,[2] was that there was a critical need for increased support of basic plasma science. If anything, this is an even greater concern today, particularly in the area of basic plasma experimentation. The panel's conclusion is that structures for the adequate support of basic plasma science are absent across most of the subfields assessed. Thus one of the panel's central findings is that if the nation's investment in plasma science is to be effectively utilized, this deficiency must be remedied. Given the stringent budgetary considerations that can be expected for the foreseeable future, any recommendation must necessarily be modest in scope and motivated by the most pressing needs. With this in mind, one of the panel's central recommendations is to point out two agencies in which increased support of basic plasma science could be of enormous benefit to the entire field.

Increased support for basic experimental plasma science in the National Science Foundation (NSF) would have great impact on a wide variety of applications. This support for broad-based basic research is most in keeping with NSF's charter, and plasma experiment is singled out because it is identified as the most critical need.

The nation's largest investment in plasma science is in the area of magnetic confinement fusion research and development, sponsored by the Department of Energy (DOE). This program would benefit enormously from increased support of basic plasma science. In addition, the DOE sponsors support for many other important energy-related applications of plasma science. Thus, the panel concludes that increased support for basic experimental plasma science by the Office of Basic Energy Sciences in DOE would provide a critical addition to the entire plasma science enterprise in the United States.

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[2] National Research Council, *Plasmas and Fluids*, in the series *Physics Through the 1990s*, National Academy Press, Washington, D.C., 1986.

The panel concludes that the most critical need is in the area of basic plasma experimentation. In order to rectify this present lack of support, the panel recommends that approximately $15 million per year be provided, and continued in future years, for university-scale experiments.

With regard to the subfields and applications assessed, the panel found that, in general, larger programs had fared better than individual and small-group efforts. Typically, it is these smaller activities that have provided a disproportionately large share of the new concepts, new inventions, and new ideas. Thus, the panel recommends reassessment of the relative allocation between larger focused research programs and individual-investigator and small-group activities.

The panel identified two steps that could be taken by the plasma science community to improve the health of plasma science in the United States. The panel concludes that there is a need for the plasma science community to work for increased "plasma literacy" in our prospective scientists and engineers. The panel recommends that the community encourage courses in basic plasma science at the undergraduate level. The panel also recommends that the community work to make the case for tenure-track recognition of plasma science as an academic discipline.

Plasma science has made significant contributions to our society and can be expected to continue to do so. The panel concludes that the few changes described above, although small in comparison with the total support of plasma science and technology in the United States, could aid immeasurably in ensuring that the potential of plasma science to contribute to our society is fully exploited.

APPENDICES

A

Federal Funding Data

As part of the development of this report, all federal government agencies that were known to fund plasma science were contacted and asked to provide funding information for FY 1989 to FY 1992. Appendix B contains the original letter of request in 1993 and the follow-up letter in 1994. Appendix C lists the agencies that were contacted.

Unfortunately, little detailed information was provided except by the military research offices, the Office of Naval Research (ONR) and the Air Force Office of Scientific Research (AFOSR). The poor response was not primarily due to lack of effort on the part of program officers, but rather to the lack of coordination and identification in agencies. This lends further support to the recommendations made in this report.

Illustrative is the effort at the National Science Foundation (NSF). There is no plasma branch at NSF, hence, no obvious coordinator for plasma science. One NSF staff member took the lead to act as an unofficial coordinator. He was able to pull together some information on funding, which illustrates many of the problems identified in this report.

In January 1993, plasma science and technology was identified as existing in seven divisions and three directorates at NSF, with a total funding of $15.4 million. To expand the assessment, a key-word search was made of all 1989 grants (the latest year for which the appropriate database is available). This led eventually to identifying all grants having at least a 5% component of plasma science or technology. These represented $17.5 million in 6 directorates and 19 divisions, and were included in 60 program elements. The largest category was space plasma with 46% of the grants, followed by plasma technology with 30%

TABLE A.1 Plasma Science Funding (current million dollars)—AFOSR, ONR, DOE

Agency	FY 1989	FY 1990	FY 1991	FY 1992	Change 1989–1992
AFOSR[a]	6.5	6.27	6.08	6.38	–1.8%
ONR[b]	~3.0	3.0	3.0	2.8	–7%
DOE-AEP[c]	3.2	2.2	2.3	1.2	–63%
DOE-ICF[d]	4.5	3.0	4.2	4.4	–2.2%
Total	~17.2	14.5	15.6	14.8	–12%

[a] Combines Life and Environmental Sciences with Physics and Electronics.

[b] Assuming 3.0 for 1989.

[c] Advanced Energy Projects, in BES.

[d] Estimate of that portion of the program going to basic plasma science.

TABLE A.2 Plasma Science Funding (current million dollars)—NASA Space Physics

Program	FY 1989	FY 1990	FY 1991	FY 1992	FY 1993	FY 1994	Change 1989–1994
SR&T	18.8	20.3	20.0	19.6	19.5	19.4	+3.2%
Data[a]	~ 10	12.7	12.9	25.7	8.0	6.9	approx. –30%

Note: SR&T = Supporting Research and Technology.

[a] The 1989 number is approximate. The large increase in 1992 combines data analysis with nonplasma instrument costs, cameras, cosmic-ray instruments, and so on, for the Pioneer and Voyager missions.

and basic plasma science with 22%. The reviewer's conclusion was that "support for plasma science and technology at NSF is *very* broad and *very* thin."[1]

With the above identified weaknesses, Tables A.1 and A.2 present a picture of the problems of plasma science funding. The programs listed in Table A.1, which fund small efforts in basic science, have not kept up with inflation, which totaled 13% from 1989 to 1992,[2] much less expanded to match the potential of

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[1] "Plasma Science and Technology at NSF," prepared by Tim Eastman, NSF Atmospheric Sciences Division, May 10, 1993.

[2] Using the consumer price index.

TABLE A.3 DOE Funding for Magnetic Fusion (current million dollars)

FY 1989	FY 1990	FY 1991	FY 1992	FY 1993	FY 1994	Change 1989–1994
345	317	284	332	327	322	–6.7%

the field. If the amounts listed in Tables A.1 and A.2 are combined with the approximately $8 million for space plasma identified at NSF, these areas receive about $40 million per year.

The final area, magnetic fusion, is described in Table A.3. Although an order of magnitude greater than that for basic plasma science, the funding for magnetic fusion also has not kept up with inflation.

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Letters to Funding Agencies

NATIONAL RESEARCH COUNCIL
COMMISSION ON PHYSICAL SCIENCES, MATHEMATICS, AND APPLICATIONS
2101 Constitution Avenue Washington, D.C. 20418

BOARD ON PHYSICS AND ASTRONOMY
(202) 334-3520
FAX: (202) 334-2791
EMAIL: BPA@NAS.EDU

February 17, 1993

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Dear Dr. ——————:

The National Research Council (NRC) has formed the Panel on Opportunities in Plasma Science and Technology (OPST) to conduct an assessment of the field of plasma science. The Panel has been charged to assess the health of basic plasma science as a research enterprise, identify opportunities in plasma science, and make recommendations to federal agencies and to the plasma science community.

As we have begun to evaluate the potential for the future, we realized that it is necessary to have a reference frame in which to work—a not-uncommon feature in physics. The specific reference frame we lack is the funding for the areas associated with basic plasma physics in recent years. Therefore, we ask your help in assembling an accurate profile of federal funding for plasma science research. It would be helpful to get this over a period of several years, in particular, over the period fiscal years 1989 - 1992. Please specify if the dollars are current or constant dollars. If the information is available, a theory versus experiment breakdown would be useful, as would any other breakout.

Attachment 1 is a list of the areas into which our panel has separated basic plasma science. This may be useful in deciding what to include in any data you can give us. Attachment 2 is a list of federal plasma science program managers to whom this request was sent. If there are others that you know of within your agency that are not included on the list, we would appreciate your forwarding a copy of this request to them.

We appreciate that this is a difficult time to make such a request, since budgets for the next year are being revised and your time is stretched thin. However, any information you can provide will be most useful to the Panel if received by March 1, 1993. Please send it to the following address:

Dr. Ronald Taylor
Panel on Opportunities in Plasma Science and Technology
Board on Physics and Astronomy
2101 Constitution Ave
Washington, DC 20418

or fax the information to Dr. Ronald Taylor, 202-334-2791.

Sincerely,

John Ahearne
Clifford Surko
Co-Chairs, OPST

Enc.: Topics List; Distribution List
cc: Dr. Donald Shapero
Dr. Ronald Taylor

TOPICS LIST

- Beams, accelerators, and coherent radiation sources
- Nonneutral plasmas
- Basic plasma science in magnetic and inertial fusion plasmas
- Space plasma physics
- Astrophysics
- Low temperature plasmas
- Fundamental plasma experiments
- Theoretical and computational plasma physics

NATIONAL RESEARCH COUNCIL
COMMISSION ON PHYSICAL SCIENCES, MATHEMATICS, AND APPLICATIONS

2101 Constitution Avenue Washington, D.C. 20418

BOARD ON
PHYSICS AND ASTRONOMY

(202) 334-3520
FAX: (202) 334-2791
EMAIL: BPA@NAS.EDU

August 12, 1994

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Dear Dr. __________:

We are writing to you as Co-Chairs of the Panel on Opportunities in Plasma Science and Technology (OPST) to ask your help in assembling financial information for a report that the panel has prepared. This letter is actually a followup to a letter that we sent to you on February 17, 1993. Some background information and our specific request are outlined below.

The OPST, under the auspices of the Board on Physics and Astronomy (BPA) of the National Research Council (NRC) has been conducting an assessment of the field of plasma science. The report is nearly complete and is currently under review at the NRC. The original charge to the panel was to assess the health of basic plasma science as a research enterprise, identify research opportunities in plasma science, and make recommendations to federal agencies and to the plasma science community. In addressing this charge, the panel assembled information on federal funding for basic plasma science. Your response to our February 17, 1993 letter was used for that purpose.

Several attachments are included for your reference. Attachment 1 is a copy of the February 17, 1993 letter that was sent to you requesting funding information on basic plasma science. Attachment 2 is a copy of your response. Finally, Attachment 3 is a copy of the section of our draft report that summarizes the information we were supplied last year. Our request is the following:

(1) We would like to update the information that you provided last year. Please review your response to us and return funding figures for FY93 and FY94 (if possible).

(2) Our original request asked for funding information associated with basic plasma science. We would like to compare these figures with the total funding for plasma science-based research. It would be most helpful if you could provide us with those figures for FY89 - FY94.

(3) Finally, please review Attachment 3 and give us any comments. We would like this section of the report not only to reflect the panel's information on the funding situation in basic plasma science, but also the funding situation of the field. Therefore, we are interested in your perspective.

We appreciate that this is a difficult time to make such a request and thank you for your time and effort. Any information you can provide will be most useful to the panel if received by September 1, 1994. Please send it to the following address:

Panel on Opportunities in Plasma Science and Technology
Board on Physics and Astronomy
2101 Constitution Ave
Washington, DC 20418

or fax the information to Natasha Casey, 202-334-2791. Ms. Casey, from the BPA office, will call you in several days to confirm that you received this request and offer any assistance that you might need in responding to this request.

Sincerely,

John Ahearne
Clifford Surko
Co-Chairs, OPST

Enc.: Attachments
cc: Dr. Donald Shapero
Dr. Ronald Taylor

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List of Agencies Contacted

- National Science Foundation
  - Physics Division
  - Astronomical Sciences Division
  - Atmospheric Sciences Division
  - Electrical and Communications Systems Division (Quantum Electronics Waves and Beams Program)
  - Office of Polar Programs (Polar Aeronomy and Astrophysics Program)
- National Aeronautics and Space Administration
  - Space Physics Division
  - Astrophysics Division
  - Solar System Exploration Division (Planetary Science Branch)
  - Goddard Space Flight Center
- Department of Energy
  - Office of Basic Energy Sciences
  - Office of Fusion Energy
  - Office of Defense Programs (Office of Inertial Fusion)
- Office of Naval Research
- Army Research Office
- Air Force Office of Scientific Research
- Air Force Cambridge Research Laboratory (Ionospheric Physics Laboratory)

NOTE: In some cases letters were sent to multiple offices or programs within those listed above.

THE U.S. PROGRAM OF FUSION ENERGY RESEARCH AND DEVELOPMENT

Report of the Fusion Review Panel

The President's Committee of Advisors on Science and Technology (PCAST)

July 1995

The President's Committee of Advisors on Science and Technology (PCAST)

President Clinton established the PCAST (E.O. 12882) on November 23, 1993. PCAST serves as the President's highest level, private sector advisory group for science and technology.

PCAST members are distinguished individuals who are recognized for significant achievements and are representative of the diverse perspectives and expertise in the U.S. science and technology establishment. They are appointed by the President and are selected from industry, education and research institutions, and other nongovernmental organizations.

The PCAST is chaired by John H. Gibbons, Assistant to the President for Science and Technology, and by John A. Young, former President and CEO of Hewlett-Packard Co.

Acting as a committee of the whole, the PCAST has authorized the transmission of this panel report to the President. The views expressed in this report are those of the panel, and individual members of the PCAST may demur from certain of its recommendations.

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20500

July 11, 1995

The Honorable John H. Gibbons
Assistant to the President for Science and Technology
The Honorable John A. Young
Former President and CEO, Hewlett-Packard Co.
Co-chairs, The President's Committee of Advisors on Science and Technology (PCAST)

Dear Dr. Gibbons and Mr. Young:

We are pleased to transmit to you the enclosed report, "The U.S. Program of Fusion Energy Research and Development." This report was prepared by the PCAST Fusion Review Panel, which was constituted by Dr. Gibbons on March 22, 1995, to review the efforts of the Department of Energy (DOE) to develop fusion as an attractive energy source. The Panel was asked to clarify the technical and policy tradeoffs and budgetary requirements associated with different options for structuring the Department of Energy's magnetic fusion energy program and to recommend a preferred option.

Our report concludes that U.S. funding for research and development (R&D) on fusion energy is a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research — plasma physics — that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and in industry. We conclude also that DOE's program plan for continuing this effort in the decade ahead represents a reasonable approach in pursuit of the National Energy Strategy goal of operating a demonstration fusion reactor by about 2025. Because it is now apparent that the budgets needed to carry out the DOE's program are unlikely to be made available, however, the Panel focused most of its attention on developing a budget-constrained fusion R&D strategy for the United States that could preserve the most indispensable elements of the U.S. fusion R&D effort, and the associated international collaboration, while spending about half as much over the next ten years as now planned by DOE.

The strategy recommended by the Panel entails hard choices, considerable pain, and difficult negotiations with our international partners in fusion energy R&D. We believe, however, that it is the best that can be done within budgets that are likely to be sustainable in the current climate, and the least that can responsibly be done to maintain a degree of momentum toward the goal of practical fusion energy and to sustain the highly productive field of plasma science. We very much hope that these recommendations will gain the support of the Administration.

Sincerely yours,

John P. Holdren

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Chairman, Fusion Review Panel

Robert W. Conn
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Enclosure

President's Committee of Advisors on Science and Technology

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PCAST Panel on the U.S. Program of Fusion Energy Research and Development

EXECUTIVE SUMMARY

Funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond. This funding also sustains an important field of scientific research — plasma science — in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable in other fields of science and in industry. And U.S. funding has been crucial to a productive, equitable, and durable international collaboration in fusion science and technology that represents the most important instance of international scientific cooperation in history as well as the best hope for timely commercialization of fusion energy at affordable cost. The private sector can not and will not bear much of the funding burden for fusion at this time because the development costs are too high and the potential economic returns too distant. But funding fusion is a bargain for society as a whole.

Based on the importance of developing energy sources adequate to meet the needs of the next century and the promise of fusion for this purpose, the benefits of fusion R&D in strengthening the national science and technology base, the impressive recent rates of progress in fusion research, the costs of the logical next steps, and the growing investments being made in fusion R&D in the European Union and Japan (which already total more than three times the corresponding investment here), we believe there is a strong case for the funding levels for fusion currently proposed by the U.S. Department of Energy (DOE) — increasing from $366 million in FY1996 to about $860 million in FY2002 and averaging $645 million between FY1995 and FY2005.[1] These are actually the minimum amounts required to support full U.S. participation in the construction phase of the International Thermonuclear Experimental Reactor (ITER) as currently envisioned while maintaining a vigorous, complementary domestic program that (a) extracts the remaining scientific value from experimental facilities already in operation; (b) constructs the Tokamak Physics Experiment (TPX) to explore crucial issues not accessible in existing devices or in ITER, and to anchor the domestic experimental program in the next century; and (c) nourishes essential efforts in smaller experiments (including alternative concepts), theory, computing, technology development, and fusion-reactor materials. It almost certainly would not be possible to spend less and still meet the fusion timetable of the National Energy Strategy, developed during the last 2 years of the Bush Administration, which calls for operation of a fusion demonstration reactor by about 2025.

Although the program just described is reasonable and desirable, it does not appear to be realistic in the current climate of budgetary constraints; we therefore have devoted most of our effort to developing a budget-constrained U.S. fusion R&D strategy that, given level funding at about half of the average projected for the period FY1996 through FY2005 under the current DOE plan, would preserve what we believe to be the most indispensable elements of the U.S. fusion effort and associated international collaboration. This strategy would cost about $320 million per year, $46 million less than the U.S. fusion R&D budget in

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[1] All dollar figures in this report are as-spent dollars unless otherwise noted.

FY1995. It would entail hard choices and considerable pain, including straining the patience of this country's collaborators in the international component of the fusion effort, forcing difficult trade-offs between even a reduced U.S. contribution to international collaboration and maintaining adequate strength in the domestic components of U.S. fusion R&D, shrinking the opportunities for involvement of U.S. industry in fusion technology development, and surrendering any realistic possibility of operating a demonstration fusion reactor by 2025. But we believe it is the best that can be done within budgets likely to be sustainable in the current climate, and the least that can responsibly be done to maintain a modicum of momentum toward the goal of practical fusion energy.

The key priorities around which our budget-constrained strategy is organized are as follows:

- **a strong domestic core program in plasma science and fusion technology**, with funds to explore both advanced tokamak research and research on concepts alternative to the tokamak, leveraged where possible on related activities worldwide;
- **a collaboratively funded international fusion experiment focused on the key next-step scientific issue of ignition and moderately sustained (circa 100 seconds) burn**, at a cost about one-third that of ITER as currently planned; and
- **an international program to develop practical low-activation fusion-reactor materials,** highly desirable for economical reactor performance and environmental attractiveness.

This budget-constrained, internationally integrated U.S. fusion R&D program would, more specifically:

- **preserve and somewhat enhance the U.S. core program in relation to its FY1995 level of about $180 million per year**, including a degree of remedy of the current program's neglect of confinement concepts other than the tokamak;
- **continue to operate, within the core program, the medium-scale tokamaks at General Atomics (DIII-D) and MIT (Alcator C-MOD), upgrading DIII-D after Princeton's Tokamak Fusion Test Reactor (TFTR) is shut down**, and continue modest funding to pursue energy applications of the inertial-confinement fusion effort being funded for stockpile stewardship purposes in the weapons budget;
- **continue to operate TFTR for 3 years beyond its currently scheduled shutdown at the end of FY1995**, at a somewhat reduced funding level of about $50 million per year; and
- **continue U.S. participation in the Engineering Design Activities phase of ITER at the current level ($70 million per year)**, to which this country is committed through FY1998 under existing international agreements.

Under this proposed budget-constrained program, the United States would also immediately open negotiations with its ITER partners to modify the post-FY1998 phase of international cooperation, seeking to

- **gain agreement for downsizing ITER construction and operation from a $10-13 billion ignition-and-long-burn physics and reactor-technology development project to a not more than $4 billion ignition-and-moderate-burn physics project**, on a construction timetable delayed 3 years from the current plan;

- **promote the possibility of significant international participation in the complementary next-generation fusion experiments hitherto planned as domestic projects** (such as TPX in the United States); and

- **add to the collaborative agenda a materials/blanket test facility, as part of the international, low-activation-materials and blanket-development program.** The United States should be prepared to commit up to a total of $200 million as its share of a project that achieves international consensus and begins construction in FY2000.

The expectation of a successful outcome from this negotiation would depend on the United States bringing to the table a firm commitment, endorsed by the President, of a $1.2 billion contribution to the next phase of the cooperation (cumulative over about 10 years). The negotiation would include the possibility of expanding the number of partners (to include, e.g., China, India, South Korea). It is possible that the outcome of the negotiation would be that the full-scale ITER was constructed despite the reduction of the U.S. contribution from what had been anticipated. This outcome would have the benefit of gaining, for the world, the additional science results and the technology-testing benefits associated with ITER as currently envisioned; but it would have the liability of sharply reducing the chance that money will be found within the international effort to fund the international materials test facility and to help pay for TPX (or another machine with a similar mission).

During the negotiation of the next phase of the ITER cooperation, construction of TPX (currently scheduled to begin in FY1996) would be delayed for 3 years. Thereafter, TPX construction would proceed *if*

(a) the outcome of the negotiation was such as to permit funding the (probably downsized) ITER ignition experiment, the materials test facility, *and* the TPX with a cumulative contribution of $1.2 billion from the United States toward the total construction costs of these three facilities, the remainder to come from our international partners,

or

(b) the outcome of the negotiation was such that the United States did not become a participant in an international ignition experiment, but an ignition experiment went forward somewhere under other auspices.

If neither of these outcomes occurred, construction of TPX would not proceed unless and until a review of the new situation — with its lack of a commitment to an ignition experiment anywhere — concluded that proceeding with TPX was the most sensible next step for the United States in that situation.

Under some of the possible outcomes from the negotiation of the next phase of international collaboration, TPX would not be built. This would be extremely unfortunate. We consider TPX to be a well conceived and innovative advanced tokamak experiment, without which the United States will lack a large tokamak of its own after TFTR is shut down. We believe, nonetheless, that the highest priority should be given to preserving both (a) U.S. participation in a robust international collaboration that includes, above all, an ignition experiment and a materials test facility, and (b) a strong domestic core program of theory and smaller experiments. If negotiation of the next phase of international collaboration under a total U.S. contribution of $1.2 billion does not produce an outcome that achieves these ends and TPX as well, then the loss of TPX will have to be considered a particularly dismaying consequence of constraining the overall U.S. fusion R&D program to $320 million per year.

In addition to developing the strategy just described for a fusion R&D program funded at about $320 million per year, we also have attempted to envision a program that could preserve key priorities at a still lower budget level of about $200 million per year. We find that this cannot be done. Reducing the U.S. fusion R&D program to such a level would leave room for nothing beyond the core program of theory and medium-scale experiments described above — no contribution to an international ignition experiment or materials test facility, no TPX, little exploitation of the remaining scientific potential of TFTR, and little sense of progress toward a fusion energy goal. With complete U.S. withdrawal, international fusion collaboration might well collapse — to the great detriment of the prospects for commercializing fusion energy as well as the prospects for future U.S. participation in major scientific and technological collaborations of other kinds. These severe consequences — deeply damaging to an important and fruitful field of scientific and technological development, to the prospects for achieving practical fusion energy, and to international collaboration in science and technology more generally — are too high a price to pay for the budgetary savings involved.

We urge, therefore, that the Administration and the Congress commit themselves firmly to a U.S. fusion R&D program that is stable at not less than $320 million per year.

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Chapter 1 THE BENEFITS OF FUSION R&D

The principal objective of the U.S. program of fusion energy research and development is to provide this country and the world with an abundant, safe, environmentally attractive, and cost-competitive new energy source. Achieving this objective would bring large benefits almost irrespective of how the energy future unfolds; and achieving it could be crucial if society finds it necessary, for environmental or political reasons, to reduce sharply the currently dominant role of fossil fuels in world energy supply.

In the course of pursuing this energy goal, fusion R&D yields an immediate and continuous additional benefit by nourishing an important branch of basic science — plasma physics — and the technologies related to pursuing it. This field of research, for which nearly all of the funding comes from fusion energy R&D budgets, has been prolific in the production of insights and techniques with wide applications in other fields of science and in industry.

Finally, for a variety of reasons, fusion energy R&D has evolved a higher degree of international scientific and technological cooperation than any other field of scientific or technological research. This cooperation — entailing not only extensive exchanges of personnel and information but also full-fledged international collaboration in design, construction, and operation of some of the largest experiments — is in itself a valuable model and precedent for internationalization of R&D in other fields. Such cooperation is likely to become increasingly important as the costs of cutting-edge R&D continue to grow in relation to the capacities of individual nations to pay for it.

In the remainder of this chapter, we elaborate, in turn, on the potential benefits of fusion R&D for the future of energy supply and on its benefits for science and technology more generally. Chapter 2 summarizes the history and current status of fusion R&D programs in the United States and elsewhere, as well as existing plans for the continuation of these efforts. Chapter 3 evaluates the current U.S. program and the Department of Energy's plans for its future against a variety of criteria. Chapter 4 offers a strategy and specific recommendations for how best to pursue fusion energy R&D under the kinds of budget constraints likely to characterize the latter half of the 1990s and perhaps beyond.

1.1 Future Energy Demand and the Role of Fusion

Energy Demand in the Mid-Twenty-First Century

Future global energy demand will be determined by rates of population growth, rates of growth in economic activity per capita, and rates of reduction in energy intensity of economic activity ("energy conservation") in the world's various regions.[1] The combination of high population growth rates and the pursuit of rapid economic development in the less developed countries means that these countries, rather than the industrialized ones, are likely to account for most of the growth of global energy demand over the next several decades. Although the details vary, most long-range projections of world energy demand show it reaching 2 to 3 times the 1990 level by the middle of the next century.[2] Getting by with the lower figure without widespread frustration of economic aspirations — and the likelihood of associated social tensions and political instability — would require assigning unprecedented priority to investments and policies that promote energy efficiency. More than a doubling of energy use between 1990 and 2050 may well be required to sustain global economic development, to foster international stability, and to facilitate investments that improve environmental quality.

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[1] If economic activity per capita is measured by gross domestic product (GDP) per person, then energy intensity is measured in energy per unit of GDP, and total energy use is governed by the identity,

energy = population x GDP/person x energy/GDP.

Of the total primary energy supplied to civilization in 1990, nearly 30% was used to generate electricity and about 70% was used in nonelectric applications of fuels (for, e.g., heat and motive power). Some 80% of the world's nonelectric energy came from fossil fuels in 1990, with most of the rest coming from biomass fuels (fuelwood, charcoal, crop wastes, and dung); of world electricity generation, which in 1990 amounted to about 11 trillion kilowatt-hours, 62% came from fossil fuels, 19% from hydropower, 17% from nuclear fission, and a bit over 1% from the sum of biomass fuels, geothermal energy, wind power, and solar energy.[3] The electric share of total energy use has been increasing: a doubling of energy use between 1990 and 2050 might well be associated with a tripling of electricity generation (to, say, 35 trillion kilowatt-hours).

Options for Meeting the Demand

The options available for meeting the world's demand for energy in 2050 and beyond are those already in use — fossil fuels, biomass energy, nuclear fission, hydropower, geothermal energy, wind energy, and solar energy — plus, potentially, nuclear fusion.[4] Each of the options already in use is likely to play some role in 2050 and beyond; each has the potential for improvement in technical, economic, and environmental characteristics beyond those associated with these options today (and each deserves investment to achieve this potential); and, at the same time, each is subject to shortcomings and constraints that could limit its contribution. In what follows, we characterize the options briefly in descending order of their current contribution to total energy supply.

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[2] See, e.g., Edmonds & Reilly (1985), WEC (1989), Holdren (1990), IPCC (1992), Johannson et al. (1992).

[3] See, e.g., Johansson et al. (1992), Holdren (1990,1994). In the United States, the shares of electricity generation were: fossil fuels 68%, nuclear fission 19%, hydropower 10%, and the sum of biomass, geothermal, wind, and solar energy a bit under 3%. Nonelectric uses of energy in the United States were supplied 95% by fossil fuels and 5% by biomass.

[4] The "hydrogen economy", which has attracted considerable interest in the context of energy for the 21st century, does not refer to an additional primary energy source but rather to a set of possibilities for using hydrogen as an energy carrier, analogous to electricity or gasoline. Just as gasoline must be made from petroleum (or, as "synthetic" gasoline, from coal) and electricity must be made from fossil fuel, uranium, falling water, or some other naturally occuring primary energy source, so also must one start with a primary (fossil, nuclear, geothermal, or renewable) energy source to make hydrogen; this can be done by thermochemical processing of hydrocarbon fuels or by the electrolysis or thermal decomposition of water.

Oil and natural gas combined accounted for 70% of all fossil-fuel use in 1990. Global resources of these convenient and versatile fuel forms are much smaller than those of coal, however, and the richest remaining oil and gas fields are very unevenly distributed. The shares of oil and gas in world energy supply — and even the absolute magnitude of their contribution -- are likely to shrink in the decades ahead. Dawdling in the task of finding supplements and replacements for them is a prescription not only for increased monetary costs associated with their increasing scarcity in relation to demand, but also for political tensions and perhaps even conflict associated with the circumstance that the largest and most valuable of these resources are concentrated in only a few regions.

Coal, which currently accounts for about 30% of all fossil-fuel use, is abundant enough to take over the burdens now borne by oil and gas — and even to expand significantly the total amount of energy derived from fossil fuel — for the entire 21st century and beyond. But coal is dirty, inconvenient, and costly to clean up or convert to liquid and gaseous synthetic fuels that could replace oil and natural gas in their main applications. Coal, oil, and gas all suffer the liability of releasing carbon dioxide to the atmosphere when they are burned, moreover, and coal is the worst of the three in this respect. (Conversion of coal to liquid or gaseous fuels aggravates this problem further, because in such conversions some of the coal's energy invariably is lost.) The possibility of unacceptable impacts on climate from accumulating atmospheric carbon dioxide might well constrain the rate at which society will want to use coal and other fossil fuels to much lower levels than would be dictated by fuel supply and costs of extraction and conversion — possibly to levels lower than today's.[5]

Energy from biomass, which currently accounts for nearly 20% of world nonelectric energy supply but less than 1% of electricity generation, has the advantage of making no net addition to atmospheric carbon dioxide as long as new growth replaces the plant materials being burned. Under current practices in many parts of the world, however, biomass use for energy is associated with deforestation, soil erosion, and acute air pollution with particulate matter and hydrocarbons. Improved practices could alleviate these problems and increase the efficiency with which biomass energy is used, but ultimately the amount of biomass energy used by civilization will be constrained by how much of the planet's land area and photosynthetic production can be spared from the requirements of providing food, fiber, biodiversity, and other services.

Hydroelectric power worldwide is half as important as biomass in total-energy terms; as a source of electricity, it is a third as important as fossil fuels and roughly equal to nuclear power. Untapped hydropower potential may equal 2 to 3 times what has been harnessed so far, but many of the untapped sites are farther from demand centers or otherwise costlier to develop than the hydro sites now in use, and there would be objections to developing many of them on grounds of environmental damages and impacts on local populations. It will be a remarkable accomplishment if hydropower is ever able to generate as much electricity as fossil fuels are generating today.

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[5] The question arises whether it is practical to use fossil fuels while sequestering the resulting carbon dioxide, rather than releasing it to the atmosphere. Carbon dioxide sequestration in depleted natural gas fields and deep aquifers, and in other ways, is one of many understudied areas of energy research. At present, the key questions of ultimate storage capacity, long-term retention of the carbon dioxide, and cost remain unanswered. Until much more work is done, it will not be possible to count on this particular technological finesse.

Nuclear fission contributed about a sixth of world electricity generation in 1990. Nearly a quadrupling of the size of the nuclear-fission enterprise worldwide would be needed in order to generate as much electricity as the world was generating from fossil fuels in 1990 — that is, this enterprise would need to grow from the equivalent of some 300 1,000-megawatt reactors today to the equivalent of more than a thousand such reactors, with associated fuel-cycle facilities. To generate half of the electricity likely to be used in 2050, nearly 3,000 large reactors would be needed. Leaving aside the demanding requirements for capital and infrastructure to implement such a scenario, whether it can be done at all will depend on whether electric-utility officials, government decisionmakers, and publics are convinced that the issues of reactor safety, nuclear-waste management, and proliferation risks can be satisfactorily managed for a nuclear-fission power complex of this scale and dispersal. This might happen, but it also might not.

Geothermal energy, windpower, and solar energy currently make very small contributions to world electric and nonelectric energy supplies, although each of these options could make larger contributions in the future.

- Geothermal energy in the forms currently harnessed is dependent on isolated, depletable deposits of hot water and steam. Using geothermal energy on a much larger scale would require tapping the hot, dry rock available everywhere at sufficient depth in the Earth's crust. The practicality and cost of doing this remain to be established.

- Windpower, which currently contributes less than a tenth of a percent of world electricity generation, is economically competitive or close to competitive with coal-fired and nuclear electricity generation at sites with good wind resources, and it could be greatly expanded. To make as much electricity as currently comes from fossil fuels would require about 10 million 300-kilowatt wind turbines (the size range that appears to be most economic).[6]

- The two solar energy technologies most likely to see large-scale use are solar-thermal and photovoltaic electricity generation. Both do best at sunny, desert sites. Photovoltaics are more versatile, as they can use the diffuse-beam solar radiation characteristic of cloudy days as well as the direct-beam radiation that most types of solar-thermal plants require; but photovoltaics are also much farther from economic competitiveness with conventional electricity sources. To generate with photovoltaic cells as much electricity as now comes from fossil fuels would require perhaps 80,000 square kilometers of photovoltaic installations, equal to about 5% of the currently urbanized area of the planet.[7] Land requirements for typical solar-thermal plants would be perhaps half as great.

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[6] This calculation is based on a capacity factor of 25%, meaning the actual annual output is 25% of what would be obtained if the plant operated at full rated capacity 24 hours per day, 365 days per year. Such capacity factors are achievable by wind turbines at good sites, by no means everywhere.

[7] This calculation is based on use of sunnier-than-average sites (annual average insolation of 200 watts per square meter, compared to the world mean of 175 watts per square meter), arrays that convert to electricity 10% of the sunlight incident on them, land area equal to twice the array area, and adequate grid-connectedness or diurnal storage to utilize all of the electricity produced.

Ocean thermal energy is another renewable energy resource of substantial magnitude, but the monetary and environmental costs of harnessing it on a significant scale are highly uncertain.

The foregoing considerations make clear that it will be an immense challenge to support a doubling or more of energy use — and, probably, a tripling of electricity use — in the middle of the next century in ways that are safe, economically affordable, environmentally tolerable, and politically acceptable. If stabilizing or even shrinking the use of fossil fuels proves to be required, as could happen for environmental or political reasons or a combination of these, the challenges posed to nonfossil sources will be all the greater.

Most of the major energy options, fossil and nonfossil alike, are subject to sharply rising costs of some kind — economic, environmental, social, political — when their scale of utilization passes a critical level. For example, hydropower, windpower, and solar energy become much costlier when it becomes necessary to resort to inferior sites; oil becomes much more dangerous politically when total demand grows so large as to require excessive dependence on the resources of unstable regions; fossil fuels altogether become much costlier environmentally when the scale of their emissions overwhelms the absorptive capacity of biogeophysical systems; nuclear fission will be problematic if it grows and spreads more rapidly than the managerial competence needed to operate it safely and to protect its fissile materials; and so on. Society's menu of energy options is also susceptible to sudden narrowing as a result of political mischief, unfavorable new knowledge, or other unforeseen developments.

In these circumstances, it should be obvious that there is great merit in the pursuit of diversity in energy options for the next century. There are not so many possibilities altogether. The greater the number of these that can be brought to the point of commercialization, the greater will be the chance that overall energy needs can be met without encountering excessive costs from or unmanageable burdens upon any one source. The potential value of developing fusion energy must be understood in this context. The possible costs of needing fusion at midcentury and beyond, but not having it, are very high.

What Could Fusion Offer?

Nuclear fusion has many attractive attributes: (a) The fuel supply is extractable from ordinary seawater (thus available to all countries) and is sufficient in quantity for millions to billions of years. (b) There are significant advantages over fission energy options with respect to possibilities for minimizing radiological hazards and links to nuclear weaponry, over fossil-fuel options with respect to emissions to the atmosphere, and over many forms of renewable energy with respect to impacts on ecological and geophysical processes. (c) The monetary costs of fusion could be comparable to those of other medium-term and long-term options. These points are discussed briefly in the following paragraphs.

Fuel supply. Extracting lithium from seawater until its concentration drops to half of today's value would yield at least 150 million terawatt-years of thermal energy in first-generation fusion reactors, based on the deuterium-tritium (D-T) reaction (where the tritium is obtained by splitting lithium). Extracting deuterium from seawater until its concentration drops to half of today's value would yield 250 billion terawatt-years of thermal energy in advanced fusion reactors, based on the deuterium-deuterium (D-D) reaction. For comparison, world coal resources are estimated at 5,000 to 10,000 terawatt-years, and world energy use was about 13 terawatt-years per year in 1990.

Safety and environment. Fusion, like fission, requires attention to the full range of nuclear safety-and-environment issues: worker safety, minimizing routine exposures of workers and the public to radiation, preventing reactor accidents, safely managing radioactive wastes, and avoiding linkages between civilian and military nuclear programs. But the issues that are most difficult to handle for fission are likely to be less so for fusion. Specifically,

- with respect to reactor safety, if priority is given in the development of fusion to achieving its potential for reduced radiological hazards, it should be possible to achieve D-T fusion reactors in which "worst case" accidents would produce population exposures to radiation about 100 times smaller than those from "worst case" fission reactor accidents; use of advanced fuels could give even larger improvements over fission;

- with respect to radioactive-waste hazards, those of fusion (based on the most meaningful indices combining volume, radiotoxicity, and longevity) can be expected to be at least 100 times and perhaps 10,000 or more times smaller than those of fission;

- with respect to links to nuclear weaponry, electricity-supply systems based on fusion would be less likely than fission-energy systems to contribute to the acquisition of nuclear-weapons capabilities by subnational groups, and would be easier to safeguard against clandestine use for fissile-material production by governments.

In comparison with renewable energy sources, fusion would have no counterpart to the ecological problems associated with large-scale production of biomass for energy (heavy use of land, water, fertilizers, and pesticides, and loss of natural biodiversity). The ecological and geophysical impacts of fusion would be less severe than those of hydropower, ocean thermal energy, and (probably) geothermal energy. In addition, fusion's land-use requirements would be smaller than those of most forms of solar electricity generation.

Economics. The cost of the raw fuel for fusion -- lithium and deuterium extracted from seawater -- would be a very small fraction of the total cost of the electricity produced. The construction costs of the power plant would account for most of the total cost (as with nuclear fission and with most forms of renewable energy). The potentially higher costs of fusion plants compared to fission plants, which are associated with the complexity of fusion technology, could be substantially offset, for the safest designs, by savings resulting from easier siting and licensing and greater simplicity in waste disposal. Safer designs could also lead to reduced requirements for "nuclear-grade" certification of plant components. Comparing the costs of electricity from fusion with the costs of electricity from renewable energy is made complicated by the base-load character of fusion and the intermittent character of many renewables, neither of which is ideally matched to an independently varying electricity demand.

If positive results from vigorous pursuit of fusion energy were to lead to deployment of the first commercial fusion reactors around the middle of the 21st century, it would be possible to imagine this source providing, by late in that century, an electricity contribution comparable to that from fossil fuels in 1990 -- about 7 trillion kilowatt-hours per year. This output would correspond to the output of nearly 600 fusion power plants of 2,000 megawatts capacity each, operating at an average capacity factor of 70%. Such plants, twice the size of today's typical coal-fired and nuclear-fission power plants, would typically be deployed on electricity grids substantially larger than today's and probably would be the largest power plants in the mix of facilities producing electricity. Contrary to most current expectations, however, fusion power plants might turn out to be cost-competitive at capacities significantly below 2,000 megawatts, in which case a 7 trillion kilowatt-hour contribution could come from a larger number of smaller plants. Smaller plants fit more easily into power grids of moderate size (which, in the mid-to-late 21st century might still characterize some developing regions), and the production of larger numbers of units provides greater opportunities for technological and institutional learning.

Because fission and fusion are both nuclear technologies and are, in some sense, competitors for the same "niche" in global energy supply — large-scale, capital-intensive, grid-connected electricity generation, with the possibility of contributing to fluid-fuel supplies by electrolytic production of hydrogen — the issue of comparisons between and possible interactions of these two technologies is particularly salient. In this context, two pathways entailing an important role for fusion can be envisioned:

- Along one path, first fission prospers, and then fusion prospers alongside it, perhaps eventually replacing it and perhaps co-existing with it indefinitely. In this scenario, the public becomes comfortable with nuclear fission before fusion is commercially available — probably in connection with reduced reactor and fuel-cycle costs, decades of trouble-free management of the nuclear fission enterprise worldwide, and decades of international diplomacy in which nuclear weapons play only a minor role. Investments in advanced fission technologies could increase the likelihood of this scenario, although they cannot guarantee it.

- Along a second path, fission does not prosper, but fusion prospers by being sufficiently different from fission in its nuclear characteristics so that either (a) these differences translate into a significant economic advantage for fusion (through, e.g., simplified safety systems, reduced regulatory requirements, and easier siting), so that fusion is economically attractive where fission was not, or (b) fusion is deemed politically acceptable where fission was not.

Pathways in which fission prospers but fusion does not, or in which neither prospers, can also be envisioned. It is not our role here to offer assessments of which of these outcomes is most likely. We want simply to point out that the possible shortcomings of other major options for the second half of the next century and beyond, the benefits of diversity in energy technologies, and the penalties attendant on inadequate energy supply are all such as to mandate continued intensive efforts to make both fusion and fission available in that time period in the most attractive forms possible.

Energy R&D Policy in Global Context

In sharp contrast to most of the countries in the world — industrialized and developing alike — the United States is blessed with abundant coal resources, oil reserves still able to supply half of domestic consumption, and gas reserves sufficient to provide the domestic fossil-fuel system with considerable flexibility. The United States also enjoys a relative abundance of land area that could provide the basis for large-scale deployment of solar-electric energy systems, biomass-energy plantations, and windfarms. Economic geography presents a much less promising energy prognosis for Japan and the countries of Western Europe, and this disparity goes far toward explaining the substantially lower levels of commitment of public resources to energy R&D per capita in the United States today, relative to the levels in these other countries of comparable prosperity.

The sense of complacency in the United States engendered by this country's relative energy-resource abundance is understandable, but it is not justified. Having suitable energy-supply options is far more a matter of having the right technology than of having the raw resources, and this will be even more true in the next century than it is today. There will be great economic benefit to the United States, moreover, if it is in a position to be an exporter of attractive energy technologies to the huge world market of the next century, and considerable economic cost if these must be imported from Japan, Europe, and elsewhere. There is particular, additional merit in exerting and maintaining leadership in nuclear energy technologies, because of the influence of choices about these technologies worldwide on the prospects for minimizing nuclear-weapons proliferation and for avoiding major nuclear accidents. This last point underlines a more general one: the United States is unlikely to be able to isolate itself from either the political turmoil or the large-scale environmental problems that will result if it turns out that adequate energy options for countries less well endowed than the United States geographically and technologically are simply not developed at all, by this country *or* by others.

Thus there is a powerful argument for the United States to invest adequately in a broad and deep program of energy research and development, to maintain a position of international leadership in this field, and to exert that leadership to foster and steer international collaboration in those aspects of energy R&D for which there are particular advantages in doing so. The U.S. energy R&D program needs to address energy efficiency and all of the major supply options and potential options, including fission and fusion; and, in the nuclear technologies especially, it needs to be closely coupled to the R&D programs of other countries. The difficulties experienced by the United States in developing and sustaining the sort of energy R&D policy that is required, and the importance of overcoming those difficulties, are treated at length in the recent report on energy R&D by the "Yergin committee" of the Secretary of Energy's Advisory Board (SEAB 1995). We hope that readers of our report will read that one, too.

1.2 Benefits of the Fusion Program Besides Energy

Advancement toward the goal of fusion power has required the birth and development of a new field of science — plasma physics. Plasmas are often referred to as the fourth state of matter (the first three being solids, liquids, and gases). It is now understood that 99% of the known matter in the universe, although little of the matter on Earth, is in the plasma state. The birth of modern plasma physics occurred with the advent of fusion research in the 1950s, and the fusion program has been the dominant driver of plasma physics ever since. Conversely, the development of plasma physics has been the engine driving progress in fusion. This essential link between fusion energy and plasma science implies that the national benefits accruing from plasma science should be viewed not simply as a "spin-off" of an energy program.

Any assessment of the fusion research program should, therefore, recognize both classes of benefits of fusion R&D — the long-range development of an energy source and the more immediate gains from plasma science. In what follows we address the latter, in two categories: first, the value of this field as science and, second, the impact of plasma science on industry and technology.[8]

Contributions to Science

Plasma physics has uncovered a panoply of new phenomena. A plasma is a gas of charged particles, each of which interacts with all the other particles in the gas — not just with those very close to one another, as in an ordinary gas of neutral particles. Understanding the behavior of plasmas poses an enormous intellectual challenge. Insight into this important medium, obtained by the techniques of modern physics, reveals that a plasma is not simply an unpredictable assembly of motions. Basic principles have been developed to understand the rich array of plasma waves, instabilities, spontaneous magnetic phenomena, and turbulence. These emerging principles have advanced fundamental concepts in complex systems, an avenue of inquiry at the forefront in numerous areas of science.

Results from fusion plasma physics have fundamental and pervasive import for many other scientific fields. In astrophysics, plasma science has been employed to understand the behavior of the plasma and magnetic fields in the earth's magnetosphere, in the sun and other stars, and in galaxies. For example, plasma physics is required to understand magnetic storms observed on Earth, solar flares, shock waves in space, magnetic fields in stars and galaxies, pulsars, accretion disks of active galactic nuclei, black holes, and star formation. Fusion plasma physics has been at the forefront in the development of the new sciences of chaos and complexity and has forged new concepts in the area of turbulence, one of the great scientific problems of this century. In the area of large-scale scientific computing, fusion researchers have pioneered the use of supercomputers to solve complex problems. In particular, the fusion energy program was the first to employ time-sharing supercomputers serving a large scientific community.

One of the applications of plasma physics has been to the non-neutral particle beams of particle accelerators. Progress in understanding and controlling plasma instabilities has made possible all modern accelerators, such as the colliders RHIC (at Brookhaven), Tevatron (at Fermilab), and PEP II and the SLC (at Stanford).

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[8] This subject is treated in far greater detail in a National Academy of Sciences report (NRC 1995) entitled "Plasma Science: From Fundamental Research to Technological Applications", which was made available to us in page-proofs while our own report was in the final stages of preparation.

Contributions to Industry and Technology

The pursuit of fusion energy has laid the scientific foundation for, and has already contributed to, a number of technologies that have applications in manufacturing, materials, electronics, electric power, computing, and the defense industries.[9]

In manufacturing, the unique properties of plasmas have led to important applications in the processing of materials. Analysis of the fundamental chemical and physical processes occurring in plasmas has led to better understanding and resulted in the improved performance of industrial plasmas. Equipment and instruments developed by fusion physicists and engineers to produce, monitor, and control plasmas have wide use. Examples of such industrial application include:

- Plasma etching, deposition, and surface modification to manufacture integrated circuits. Plasma processing is a principal manufacturing technology for creating microelectronic devices on the very small (submicron) scale that is required for the advanced integrated circuits in computers, communications equipment, and consumer electronics products. This technology also reduces toxic wastes from microelectronic-circuit manufacturing.

- Plasma-assisted chemical vapor deposition to prepare diamond and superconducting films. Models to optimize film growth require knowledge of molecular dynamics and film microstructure that are a result of developments in plasma processing and materials characterization.

- Plasma-ion implantation to harden tools, to produce anticorrosion coatings, and to reduce wear by creating low-friction surfaces for both industrial and biomedical applications. Ion implantation treats the surface of metal parts, such as high-strength ball bearings, cam shafts for performance vehicles and military equipment, and prosthetic joints that are low-friction and biocompatible.

The needs of fusion research have provided a major stimulus for the development of superconducting magnets. In order to confine very hot plasmas, superconducting magnets of unprecedented size and power have been required. Applications of superconducting magnets are potentially extensive; they include energy storage, transportation, and rocket propulsion.

Several classes of advanced materials have been developed in the course of fusion and plasma research. Superconducting wire and cable configurations are now used in various industrial and medical applications. High-strength, nonmagnetic steels, composites capable of withstanding very high heat fluxes, new surface-cleaning methods, and new electron-beam welding techniques — all developed in fusion research — are finding numerous applications.

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[9] For additional detail, see, e.g., USDOE (1993).

An important contribution of fusion has been the advancement of pulsed-power technology, including capacitors, switches, and cables, to meet the high power needs of fusion devices. Applications include pulsed-power components and systems used for defense and commercial research and applications. Lightweight, compact, and reliable power supplies, initially used for tokamak plasma control, are being used in defense and in rail transport.

Many practical engineering computation and simulation techniques can be traced to the fusion program in computational physics. For example, both the computational methods to describe the magnetic field generated by complicated coils and the finite-element method of analysis of stress were developed for the design of magnetic fusion devices. These codes have been adapted by engineers working on magnet designs for electromagnetic launch and levitation systems. As another example, the computational solutions of the electromagnetic wave equation, developed in plasma heating studies, are being used in engineering applications ranging from antenna design to calculating radar cross sections.

The above examples are only illustrative. The influence of plasma science and fusion-related technology is growing, with many applications only now emerging. Already there are a considerable number of successful spin-off companies, which are transferring important technologies to several commercial sectors. A strong fusion program would continue to be an important driver in this area.

Chapter 2 HISTORY, STATUS, AND CURRENT PLANS

The U.S. fusion R&D program is best understood in terms of the history of fusion R&D, key issues that such R&D must address, status and plans of the current (FY1995) U.S. program, the U.S. funding picture, fusion R&D activity in other countries, the role of international cooperation in fusion R&D, and the pace of progress. In this chapter, we treat these topics in a largely descriptive way. Evaluation follows in Chapter 3.

2.1 History of Fusion R&D

The history of work on fusion energy, together with extensive descriptions of the various experimental fusion machines that have played important roles in the development of this field, can be found in more detail than is possible here in a number of recent major reviews of U.S. and world fusion programs.[1] What follows here is a capsule summary.

Fusion energy R&D began in 1951, in Britain, the United States, and the Soviet Union, as a spin-off of weapons work on the hydrogen bomb. The work was kept secret until 1958, when work on magnetic fusion energy (MFE) in all three countries was declassified by an agreement reached at the 2nd United Nations Conference on Peaceful Uses of Atomic Energy, held in Geneva. Since then, there has been excellent international cooperation in MFE; this cooperation has, in fact, been more extensive and important, in relation to the total amount of activity in the field, than collaboration in any other field in science or technology.

In the 1950s and 1960s, many different configurations for containing plasmas were considered. At that time, physicists realized that the development of a "magnetic bottle" was the primary subject (it still is), and they believed that a variety of approaches would be successful. Stellerators, mirror machines, Z-pinches, q-pinches, and other devices were studied. Slowly, a sophistication was developed, both experimentally and theoretically, through the development of a diagnostic ability that allowed careful measurement and through the development of the theory of plasmas. Physicists began to understand the subject of instabilities. Soon they were able to comprehend, and even predict, which devices would be subject to what type of instability and why.

During this period, the basic parameters needed to measure progress towards harnessing fusion energy were identified. These parameters comprise the ion temperature (T_i), the product of plasma density and confinement time ($n\tau$), and the power produced divided by the power put into the plasma (Q). Achieving a significant fusion reaction rate using the most reactive fusion fuel (a mixture of deuterium and tritium) requires T_i greater than 100 million degrees Celsius. Achieving a reaction rate sufficient to produce more energy than needed to heat the fuel ("energy break-even") requires, in addition, that the product $n\tau$ exceed 10^{14} seconds times fuel ions per cubic centimeter (called the Lawson criterion). Finally, Q must be considerably larger than unity to make up for energy lost to infrastructure (e.g., pumps, fans, lighting), to the inefficiency of the devices that heat the plasma (gyrotron tubes, neutral-beam injectors, etc.), and to the production of the magnetic fields of the "bottle" (resistive losses in the magnet coils and/or power for refrigerators if the coils are cryogenic or superconducting). "Ignition" corresponds to infinite Q, but even a finite Q that is large compared to unity would be of interest for fusion energy.

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[1] See, e.g., OTA (1987), NRC (1989), ERAB (1990), FEAC (1992), and OTA (1995).

In 1968, Soviet scientists announced that they had achieved very long confinement times of hot, dense plasmas in a tokamak device — called T-3 — which had been conceived by Sakharov and Tamm.[2] The fusion world was astounded, but skepticism was laid to rest when a British team, invited by the Soviets to independently make measurements of plasma parameters in T-3, confirmed the announced results. This event initiated a worldwide effort on tokamaks that blossomed in the 1970s and into the 1980s.

In the United States, this effort included the achievement of a record Lawson parameter on Alcator (MIT) in 1975 and the authorization of Doublet III (General Atomics) and the Tokamak Fusion Test Reactor (TFTR; Princeton) in the same year. Record temperatures were achieved in the Princeton Large Torus (PLT) in 1978. Meanwhile, work proceeded in a number of U.S. laboratories on fusion experiments in configurations other than tokamaks — mirror machines, pinches, and so on (discussed further below) — at a somewhat lower level of effort.

In 1980, the Magnetic Fusion Energy Engineering Act authorized a 20-year, $20 billion effort, but it never came to pass. The annual appropriations needed to implement it simply did not materialize. Instead, throughout the 1980s, projects investigating confinement concepts other than the tokamak were terminated one after the other to make room, in declining budgets, for continuation of a vigorous effort on tokamak development. Stimulated in part by the combination of budgetary stringency and the escalating costs of proceeding further down the tokamak line of development, there emerged from the 1985 Reagan-Gorbachev summit an agreement to intensify international cooperation in fusion research in the form of a collaborative project to design an International Thermonuclear Experimental Reactor (ITER). That project, which is described in the next section, was proposed as a major tokamak that would address both physics and engineering issues.

Although a number of proposals to construct new U.S. tokamaks — such as the Compact Ignition Torus (CIT) and the Burning Plasma Experiment (BPX) — were submitted in the late 1980s, none was funded. The domestic experimental effort focused, in the 1980s and the first half of the 1990s, on operating and, in some cases, upgrading the tokamaks that had been authorized in the 1970s:

- In 1983, Alcator achieved $n\tau$ adequate for fusion, but the plasma temperature was too low for energy break-even. An upgrade of this machine was authorized in 1987 (Alcator C-MOD).
- Doublet III and its upgrade (DIII-D) produced many significant results, including demonstration of high-quality plasma-confinement regimes (suitable for advanced tokamaks), divertor concepts (of great importance to many fusion configurations), and high-power microwave heating.

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[2] The term "tokamak" is a Russian acronym for words meaning toroidal chamber with magnetic coils.

- TFTR produced several significant accomplishments. Learning how to handle tritium and learning how to perform the everyday operation of the device were necessary. At the same time, sophisticated diagnostics, which now make this machine perhaps the best equipped fusion device in the world, were developed. In 1994, TFTR achieved the record fusion output power of 10.67 megawatts and Q=0.3. The detailed study of plasma-wall interactions, the self-sustaining bootstrap current, alpha-particle heating and removal, and the production of reversed shear (for enhanced confinement) have been of equal importance.

Investigation of the tokamak concept has also dominated the world's other major fusion R&D programs over the past two decades. In Russia, the T-3 was succeeded in the mid-seventies by the larger T-10 and later by the T-15.[3] European researchers built the ASDEX tokamak in the seventies and Tore-Supre and Frascati Tokamak Upgrade (FT-U) tokamaks in the eighties; and, collectively, the European fusion programs completed the Joint European Tokamak (JET), a D-T-fueled machine somewhat larger than TFTR, in 1983. Japan established a vigorous fusion R&D program in the 1970s, leading to operation of a large tokamak called JT-60 in 1985. Russia, various European countries, and Japan also maintained smaller-scale investigations of magnetic-confinement concepts other than tokamaks, and the non-U.S. efforts in these alternative concepts remain stronger than those in the United States today. (Current U.S. and non-U.S. programs are reviewed in more detail in subsequent sections.)

The worldwide effort in MFE was paralleled, beginning in the 1960s, by a narrower effort along an alternative pathway to controlled fusion called inertial fusion energy (IFE). IFE entails using pulses of laser or particle-beam energy to compress and heat small pellets of fusion fuel to ignition conditions, whereupon the duration of the burn is limited by the rapid expansion of the reacting fuel, constrained only by inertia. Break-even occurs, as in MFE, when the energy yield exceeds the energy deposited on the pellet by the laser or particle beam.

In contrast to the case of MFE, the physics of IFE resembles in some respects the physics of thermonuclear weapons. Consequently, most of the U.S. effort on IFE has been directed toward the study of weapon physics and has been funded, accordingly, out of the defense budget rather than out of the energy budget, and much of the work has been classified. The largest IFE efforts outside of the United States have been in other nuclear-weapon states, notably the Soviet Union and France, and appear to have been similarly motivated. The possibility of harnessing this approach as an energy source is of considerable interest, however, and a small part of the Department of Energy (DOE) fusion energy budget is currently devoted to exploring IFE's energy applications.

Very substantial progress in IFE research has been made since the first encouraging results were obtained in 1969. Most of the effort has been based on use of powerful lasers as the source of the energy deposited on the fuel pellet, culminating with the very large Nova laser facility completed at the Lawrence Livermore Laboratory in 1985. In addition, an important set of "proof of concept" experiments was done using underground nuclear explosions as the energy source for compressing and heating fuel pellets, but this approach is not relevant to reactor possibilities. Many IFE experts question whether lasers will lead to commercial reactors, either, and advocate exploring the use of heavy-ion beams instead. Such heavy-ion "drivers" for IFE have received relatively little funding to date, although a number of national reviews of IFE efforts have recommended increased emphasis on this approach.

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[3] T-15, although completed, has not operated because of financial difficulties in the Russian program attendant on the economic changes ongoing in that country.

The terms of reference of this review dictated a focus on the fusion R&D supported by the DOE Office of Fusion Energy (OFE) — hence mainly on MFE rather than on the inertial-fusion work funded from the defense budget — but we do give some attention to the value of continuing to explore inertial fusion's potential application as an alternative route to commercial energy.

2.2 Key Issues in Fusion R&D

Pursuit of the promise of commercial energy from magnetic fusion requires an R&D program embracing four key elements: core plasma science and fusion technology; ignition and burn; steady-state operation; and materials development. In the following subsections, we indicate briefly what each of these elements entails.

Core Plasma Science and Fusion Technology

A broad core research program is necessary to address the multitude of scientific issues and opportunities presented by the pursuit of fusion energy. The nature of the scientific issues requires a spectrum of devices; some scientific issues are best addressed in large experiments, some in small experiments. What is understood as the core program in the U.S. fusion community today is the part of the fusion R&D effort *not* associated with the largest experimental devices that are operating or are under design or construction — that is, at present, the program other than TFTR (operating), ITER (in design), and TPX (ready for construction).

The core program provides scientific underpinning for the fusion endeavor and for the generation of new ideas, which are essential to progress. It is also the source of much of the support for graduate teaching and postdoctoral training in plasma physics and fusion technology, which are essential to the future vitality of the fusion-energy effort. It includes research into improvements to the tokamak concept, exploration of alternatives to tokamaks, basic theory and computation, basic experimental studies, and fusion-technology research, as elaborated in the paragraphs that follow.

Tokamak improvements. The tokamak fusion concept is highly developed, to the point that a tokamak ignition experiment is feasible. The prospects for turning a tokamak into a commercial fusion reactor are hampered, however, by specific drawbacks relating to its economic attractiveness. Among these shortcomings of tokamaks are their vulnerability to disruptions (spontaneous events in which the plasma energy is rapidly lost to the wall), their need for electrical current in the plasma (to produce a portion of the magnetic field that confines the plasma), and their low power density (and consequent large physical size and high cost). These drawbacks may prove susceptible to alleviation through research. Techniques to control disruptions, and improved methods of current drive, can probably be investigated by combining medium or small tokamak experiments with theory, as part of the core program. Approaches to achieving higher plasma pressure have been identified that would make use of a predicted alternative stability regime accessible through plasma shaping and sophisticated current control; these approaches can also be investigated to some extent through small- and medium-scale experiments in the core program, but they will ultimately require testing in larger devices capable, variously, of ignition and steady-state operation (discussed below).

Alternative concepts. Progress in fusion has been driven historically by a strong evolutionary process involving parallel exploration of various confinement concepts. In this way, new ideas arise, less favorable ideas die, concepts continually improve, several fusion concepts sometimes merge into a new approach, and fusion science advances. There is a nearly continuous spectrum of alternative magnetic-confinement concepts, ranging from those that are close relatives of the tokamak to those that are radically different. Today, those that are closer to the tokamak are often moderately developed, and those further from the tokamak tend to be much less highly developed. (The principal exception is the inertial-fusion alternative, which, as a nonmagnetic approach, is very far indeed from a tokamak but is quite highly developed, both theoretically and experimentally, because of work done in the defense program.) Research into a specific alternative fusion concept is usually motivated by a perception that the concept has a possibility of evolving into a more attractive reactor than a tokamak; in addition, alternative configurations often permit investigation of valuable fusion plasma physics under conditions not possible in a tokamak. Attractive features of alternative concepts currently under study worldwide include the absence of plasma current and the associated need for current drive (such as in the stellarator); reduction in the magnetic-field requirements for confinement (the reversed-field pinch — RFP — and spherical torus); and extreme compactness (the spheromak and field-reversed configuration). Each alternative concept has advantages and disadvantages, and each presents scientific issues that must be addressed before its reactor potential can be adequately assessed.

Basic theory and computation. Theoretical and computational studies of fusion plasma physics are needed to attack all aspects of the fusion problem, from understanding of basic processes, to development of new alternative concepts, to design support for large devices. Theory has enjoyed great success in many areas, including the development of predictive capability in macroscopic plasma stability, electromagnetic wave heating, and current drive. Such theoretical advances have been used to improve the tokamak and to evolve alternative concepts. Enormous challenges remain in many areas, such as plasma turbulence and transport. Advanced computational techniques, including plasma simulation, are needed to treat the complex, nonlinear equations that describe plasma behavior, and computational physics has taken on importance equal to that of analytical methods. Theoretical and computational studies are relatively low-cost activities with enormous impact on the fusion program.

Basic experimental studies. Basic experiments, focused on specific scientific issues, constitute a modest but valuable component of the core program. Such experiments are typically not performed in experimental configurations suitable for a reactor. Their purpose, for example, is to isolate a particular physics issue and optimize the experimental design for study of that issue, rather than to test a fusion-reactor concept.

Fusion technology research. A fusion reactor will strain the limits of current technology in several ways. As these technological limitations are addressed and gradually overcome, the fusion-reactor concept itself may change. Three particularly important technological issues are fusion-reactor materials, plasma-control systems, and the interaction of high-temperature energy conversion and tritium management:

- The development of suitable materials for a fusion reactor — especially materials that resist damage and activation under bombardment by fusion neutrons ("low-activation materials") — is perhaps the dominant technological issue. Many aspects of materials studies, such as preliminary development and testing of new low-activation materials, can be carried out within the confines of the core program. Because materials testing will ultimately require a major facility for the generation of large quantities of neutrons, however, the materials effort must eventually outgrow the core program; this larger dimension of the materials issue is discussed separately below.

- Plasma-control systems include heating and current-drive systems (such as radiofrequency waves and neutral beams) and fueling systems (such as pellet injectors). Large experiments and reactors will require advances in these systems to function in steady state and at high output. In addition, a tokamak fusion reactor will require very large, state-of-the-art superconducting magnets.

- In a fusion power reactor fueled by the D-T reaction, a "blanket" located between the plasma core and the superconducting magnets will have the dual functions of (a) regenerating tritium by means of reactions between the fusion neutrons and lithium and (b) transferring the energy of the neutrons to a high-temperature fluid medium from which the energy can be converted, outside the blanket, to electricity. The tritium is radioactive and, as an isotope of hydrogen, diffuses readily through many metals at high temperatures. Combining the functions of high-temperature energy conversion and adequate tritium production and containment poses large technical challenges.

Another important element of fusion-technology research is systems studies. These studies conceptualize a full reactor plant and examine the effects of different features on reactor attractiveness. They are extremely valuable, not as precise cost predictors, but as guides to the research themes that might have significant impact on the reactor product.

Ignition and Burn

In all existing MFE experiments, the plasma is heated by external sources. The external energy input is required to overcome inevitable leakage of energy out of the plasma. Ignition denotes the condition in which fusion conditions are self-sustaining: The alpha particles produced in the fusion reaction deposit their energy back into the plasma at a rate sufficient to keep the plasma at a fixed temperature. At ignition, the external heating can be turned off and the plasma will undergo fusion "burn" continuously.

In an ignited plasma, the alpha particles can influence the plasma behavior in ways that are difficult to predict. The difficulty is that important aspects of plasma behavior depend on plasma waves and turbulence, the description of which is at the forefront of theoretical physics research. If the alpha particles interact with the plasma in a simple fashion (for example, without influencing plasma turbulence), then simple, well known calculations will predict the behavior of an ignited plasma. If the alpha particles alter the turbulence, however, or if their interaction with the plasma is affected by the turbulence, then current theory cannot supply reliable predictions. The kinds of calculations that are currently practical provide insight into possible new effects, but not predictive capability.

Three related issues of great interest are alpha-particle heating, alpha-particle transport, and alpha-particle-generated instabilities. It is desired that the alpha particles effectively heat the plasma so as to achieve ignition (the heating problem), that they be confined long enough to do this (the transport problem), that they be confined briefly enough that they do not dilute the fuel (the transport problem again), and that they not generate new plasma turbulence that can degrade confinement (the instability problem). The only definitive way to determine the behavior of an ignited plasma is through an ignition experiment, aided by theory. Two-fold differences in outcomes can have a dramatic effect on the prospects for fusion.

The current experiments in TFTR provide a useful example. TFTR is far from ignition, but it is generating a population of alpha particles that is large enough to explore some of the effects that alpha particles will produce in fusion reactors. The alpha particles deposit their energy into the plasma exactly as predicted by simple theory, but the plasma confinement actually improves in the presence of alpha particles. There is about a 25% improvement in the confinement time of the plasma, a completely unanticipated result that is not yet understood.

Producing an ignited plasma will be a truly notable achievement for mankind and will capture the public's imagination. Resembling a burning star, the ignited plasma will demonstrate a capability with immense potential to improve human well-being. Ignition is analogous to the first airplane flight or the first vacuum-tube computer. As in those cases, the initial model need not resemble the one that is later commercialized; much of what would be learned in a tokamak ignition experiment would be applicable both to more advanced tokamak approaches and to other confinement concepts.

An unfortunate distinguishing feature of ignition, relative to analogous seminal demonstrations in other fields, is that it will be very expensive to achieve. The cost exceeds what the United States and, probably, other nations individually are able or willing to expend. All of the leading nations in fusion research have taken the position, however, that achieving ignition is of high importance. These circumstances underlie the international decision to pursue ignition in the collaborative ITER project.

Steady-State Operation

Existing large tokamaks produce transient plasmas. Typically, the plasma duration is in the range of 1 to 10 seconds (although the proposed upgrade of the Tore Supra tokamak in France may be capable of a 1,000-second pulse). A power plant will need to generate energy continuously. Thus, it is essential that the physics and technology of fusion plasmas be tested under steady-state conditions.

From a physics viewpoint, one can argue that the plasma duration need not be truly continuous, but only larger than the longest physical time scale of interest. Many plasma physics time scales of significance, such as instability time scales, are very short — less than one-thousandth of a second. The longest plasma physics time scale of interest is thought to be the time for the plasma current to equilibrate. This time can be in the range of 100 seconds for large experiments (depending on the parameters of the particular experiment). Seeking a factor-of-ten margin beyond this level, then, would entail a plasma duration of about 1,000 seconds. The duration of most present experiments exceeds all plasma physics time scales except the current equilibration time.

An important feature of fusion experiments that extends beyond pure plasma physics considerations, however, is the interaction of the plasma with the surrounding structure. The plasma energy deposition on material surfaces releases impurity atoms into the plasma. The impurity atoms can cool the plasma and alter its behavior. The time scale for the plasma and wall to reach an equilibrium is difficult to calculate. It depends on both plasma physics and solid-state physics in complex ways. Thus, a definitive investigation of plasma behavior for fusion application requires a steady-state experiment.

As the plasma duration becomes long, the combined physics and technology issue of handling the intense energy flux to the wall becomes critical. In transient plasmas, the total energy leaving the plasma can be handled with existing materials and energy-channeling (divertor) methods. At long duration or steady state, however, the energy flux challenges the capabilities of currently available materials. Definitive tests of power-handling techniques require plasmas of long duration.

Steady-state operation is, therefore, an important milestone for fusion research. The large stellarator under construction in Japan will operate in steady state and provide valuable information, much of it transferable to tokamaks. It is also critical to examine advanced tokamak scenarios under steady-state operation. Finally, from the viewpoint of the electric-utility industry — the ultimate customer — the credibility of fusion power requires a demonstration of steady-state operation; physics arguments relating to time scales are insufficient.

Materials Development

The materials from which the reactor structure is made are critical in several respects. Fusion is distinct from fission in that the products of the fusion reaction are not themselves radioactive. The surrounding reactor structure can become radioactive, however, as a result of "neutron activation" reactions caused by bombardment of the structural materials by the neutrons released in the reaction. Complete realization of the environmental advantage of fusion requires the development of materials that yield little long-term radioactivity from these neutron-activation reactions. Fortunately, even with current materials the radiological hazards posed by a fusion reactor would be smaller than those posed by a fission reactor of similar electrical output; but the opportunity exists to increase this advantage by orders of magnitude if new materials under consideration prove suitable.

In addition to considerations of hazard minimization, it is necessary that materials retain their structural integrity after being subjected to years of intense fluxes of energy and neutrons. Development of appropriate materials to meet these structural requirements, not to mention hazard-minimization goals, will eventually require a neutron source to simulate the conditions of a fusion reactor. This could take the form of either an accelerator-based neutron source for small component tests or a volume neutron source (a fusion plasma) for more realistic large component tests. A facility of either type would be large and expensive, and, accordingly, filling this materials-testing need has been under discussion as a potential focus for international collaboration.

2.3 Status and Plans of the Current (FY1995) U.S. Fusion Program

The current program, at an annual budget of $365 million, is composed of seven components. In order of decreasing size, these components are: moderate- to large-scale tokamak experiments, ITER design, the Tokamak Physics Experiment (TPX) design, small-scale fusion physics experiments, technology development, theory and computation, heavy-ion inertial fusion, and other small activities. (See Table 1.)

Table 1. FY1995 Fusion Budget Breakdown

Program Component	Funding (million FY1995$)	Percent of Budget
Moderate- to large-scale tokamaks	139	38
ITER	71	18
TPX	42	12
Small-scale experiments	24	7
Technology research	23	6
Theory	17	5
Inertial fusion energy	9	2
Other	39	11

The activity in moderate- to large-scale tokamaks (38% of the budget) is concentrated mainly in three experiments: TFTR at Princeton, DIII-D at General Atomics, and Alcator C-MOD at M.I.T.

- TFTR, which began operation in 1975, is a tokamak of circular plasma cross section operating with D-T fuel. Recent TFTR experiments have produced nearly 11 megawatts of fusion power for a duration of about 1 second per pulse, permitting study of the behavior of the reaction-product alpha particles in the plasma. These results have been widely celebrated and constitute a significant milestone for fusion research. TFTR is scheduled for shutdown by September 1995, although the TFTR group has described a possible several-year extension that would probably yield 20 megawatts of fusion power and, more importantly, would enable further study of the dynamics of plasmas under the influence of alpha particles. Operation of TFTR will cost $66 million in FY1995.

- After the termination of the TFTR project, DIII-D will be the largest operating tokamak in the United States. Although not capable of D-T operation, it is of more modern design than TFTR and well suited for forefront research in the advanced tokamak physics that may lead to tokamak reactors with higher power densities, hence smaller size and lower cost for a given output. Issues being studied in DIII-D include pressure limits, energy and particle transport, disruptions, self-driven currents, current-profile control with radiofrequency current drive, and divertor studies. The DIII-D group proposes to upgrade the facility to further these studies. Operation of DIII-D will cost about $40 million in FY1995.

- Alcator C-MOD is a compact, high-magnetic-field, high-power-density tokamak. It is the most recent of a sequence of experiments that have exploited high magnetic field to advance fusion plasmas to new parameter regimes. It began full operation within the past year. Its compact size yields a very high surface power flux; thus, the device is well suited to explore the physics and operating modes of divertors. Such studies are likely to have application not only to tokamaks but also to alternative concepts. In addition, external radiofrequency heating at high power is available on C-MOD, and this machine can also contribute to advanced tokamak studies through its profile-control capability. Operation of Alcator C-MOD will cost about $16 million in FY1995.

As indicated above, we consider DIII-D and Alcator C-MOD to be part of the U.S. "core program" in plasma science and fusion technology, whereas TFTR — like the ITER and TPX projects we describe next — is treated as a large project outside the core.[4]

ITER is an international venture in which the costs, contributions, and benefits are shared among the European Union, Japan, Russia, and the United States. Currently in the engineering-design phase, it is a very large experiment aimed at testing, in an integrated fashion, the physics of ignited and sustained-burn plasmas as well as all the key technologies needed for a fusion power plant. It is the only experiment currently planned anywhere in the world that will attack the important physics issues of ignition and sustained fusion burning. It is being designed to produce about 1,500 megawatts of fusion power during a pulse length of 1,000 seconds, with the possibility of later upgrade to steady-state operation.

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[4] In some reviews, TFTR is included in what is called "the base program" and ITER and TPX are the only projects listed outside the base.

In the technology-testing phase that is to follow an initial period of physics experiments, ITER is expected to explore such critical technological issues as the performance of high-heat-flux components, the behavior of blankets that must recover the heat of the neutrons for conversion to electricity, the response of materials under neutron bombardment, the performance of large-scale superconducting magnet systems, and the operation of the complete fusion fuel cycle, including the reprocessing and recycling of tritium fuel. ITER will not be a power plant, however, and it is not intended to generate electricity. It is a physics and technology experiment aimed at testing all key aspects of a practical fusion energy system, intended in the DOE's program plans to be the step prior to a demonstration power plant. DOE also stresses the value of ITER as a pioneering model of international collaboration, from conception to construction, in large science and technology projects.

The ITER conceptual design is complete, and the 5-year Engineering Design Activity (EDA) is under way, with completion expected in 1998. The integrated total EDA cost is about $1 billion, split four ways. The total project cost,[5] including construction, has not yet been set but is anticipated to be $10-13 billion, distributed over 8 to 10 years. This latter cost will not be split four ways but is to be negotiated among the four parties: the host probably will be paying greater than a one-quarter share of the total costs, with the other partners paying less than a one-quarter share. No decision has been made yet about ITER construction; the negotiations on this subject are expected to take place over the next 2 to 3 years. If the ITER partners decide to proceed with construction on the schedule currently envisioned, plasma operation will probably commence in about 2008. At present, ITER EDA activities constitute about 19% of the U.S. fusion program, although other components of the program also perform some work that supports ITER.

TPX, a national facility presently under design, is to focus on investigating advanced tokamak physics in steady state. Compared to ITER, which is a relatively conservative physics design that would extrapolate to a large and quite costly reactor, TPX is aimed at producing the understanding leading to a more compact, economically attractive tokamak power plant. Unlike all large tokamaks to date, which operate for only several seconds, TPX will be able to operate continuously. It will not, however, use tritium fuel, except in tests toward the end of its experimental lifetime. The steady-state feature will establish whether the advanced tokamak operating modes tested to date for several seconds will persist for long time periods. TPX is to be situated at Princeton, in TFTR's present location. As a national facility, however, its management and operation will be shared by institutions spanning the U.S. fusion community, as its design has been. Already, industrial participation has been significant and at a level not found in previous fusion efforts. In FY1995, TPX design constitutes 12% of the fusion budget. Its total project cost is estimated by the DOE at about $750 million. The start of TPX construction awaits a determination of priorities for the future of the U.S. fusion program.

About 7% of the U.S. fusion R&D budget in FY1995 is allocated to small experiments, i.e., those with annual operating costs of less than $5 million. This work mainly involves tokamak experiments studying basic physics processes critical to fusion, such as plasma transport, as well as novel ideas to improve the tokamak, such as new current-drive techniques or transport-suppression techniques. Many of the small experiments are located at universities.

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[5] The term "total project cost" means, in conventional usage in this field, the cost of completing the facility but not the cost of operating it.

About 6% of the FY1995 budget is allocated to technology research, which encompasses work on the development of advanced materials suitable for a fusion reactor environment, plasma technologies (such as neutral-beam systems, radio-frequency sources, and fueling systems), fusion technologies (tritium handling, neutronics), and reactor-systems studies. A substantial amount of general fusion technology R&D is also under way as part of the ITER effort described above.

Theoretical studies of fusion plasma physics make up 5% of the U.S. program. These studies address all aspects of the fusion problem, from understanding of basic processes to development of advanced tokamak concepts to ITER support studies. The theoretical program involves national laboratories, universities, and industry.

Studies of IFE using heavy ions as the driver (long believed to be the most likely IFE concept to lead to a commercial fusion reactor) receive only 2% of the fusion energy R&D budget. Work on inertial-confinement fusion in the U.S. defense program, which is based mainly on laser drivers, was funded in FY1995 at $176 million. This spending level will increase substantially if the National Ignition Facility (NIF), a next-generation laser/inertial-fusion experiment to be built at the Lawrence Livermore National Laboratory as part of the (weapon) Stockpile Stewardship Program, goes forward as currently planned. NIF will cost about $1 billion to build, and construction is to start in FY1997.

2.4 Current U.S. Fusion Funding in Context

Expressed in constant dollars, U.S. outlays for fusion energy in the first half of the 1990s have averaged $330 million (1992 dollars) per year, which in real terms is about half of the level of a decade earlier (see Figure 1). Measured in as-spent dollars, U.S. funding for fusion energy research was about $30 million (as-spent dollars) per year from the late 1950s to the early 1970s, rose steeply to about $450 million per year in the mid-1980s, then fell to about $300 million per year in the late 1980s. U.S. government outlays for fusion energy R&D up to 1990 totaled about $13 billion (1992 dollars). At the current spending level, the U.S. government's outlays for fusion R&D represent about 20% of total government spending for energy-supply R&D (fossil, renewable, geothermal, fission, fusion, and end-use efficiency combined[6]) and are equivalent to about 0.15% of U.S. electricity revenues in this country.

The current U.S. fusion program plan is based on the assumption of strong future budget growth. The allocation of significant design funds for ITER and TPX will reap a significant return only if the experiments are constructed and operated. The importance of these experiments is emphasized in the DOE budget request for FY1996. Significant differences between the FY1995 budget and FY1996 plans include the following: U.S. ITER design activity increases from $70 million to $85 million, TPX increases from $42 million to $62 million as it begins construction,

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[6] It is reasonable to include energy end-use efficiency with energy "supply", because a kilowatt-hour saved by increasing efficiency is fully as useful elsewhere in the economy as a kilowatt-hour generated by a power plant.

TFTR is terminated (with some $40 million allocated to initiation of TFTR shutdown activities), and the small experiment program decreases from $24 million to $21 million.

Thus, the core research program is currently being limited so as to provide funds for TPX and ITER design. If the United States decides to proceed as a full partner in ITER, this country's annual outlays for this activity after 1998 will peak at about $500 million. TPX expenditures for construction will reach about $135 million annually. The DOE program plan to accommodate these two large experiments plus a core research program calls for annual fusion budgets increasing from $366 million in FY1996 to $860 million in FY2002 and then falling to $700 million (all as-spent dollars) in 2005. The program-plan average for the 10 years from FY1996 to FY2005 is $645 million per year. The strong probability that fusion R&D budgets of this magnitude will not actually be forthcoming is, of course, the primary reason that PCAST was asked to convene a panel to review the current fusion R&D program and plan, as well as less costly alternatives.

2.5 Current Status and Plans of Non-U.S. Fusion Programs

The other dominant players in fusion research are the European Union, Japan, and Russia. The European Union's fusion R&D program is now nearly twice as large as that of the United States, and Japan's program is also larger than ours; together, the European Union and Japan spend about three times as much on fusion as the United States does. These other programs also exceed that of the United States in breadth and, apparently, commitment to long-term stability in funding for fusion R&D.

The current funding level of fusion R&D in the European Union — which is also the level projected for the next 4 years — is the equivalent of about $600 million per year. The lead tokamak in Europe, JET, is located in England. JET is larger than TFTR and contains more advanced tokamak features. Several years ago, tritium was introduced to JET to produce 2 megawatts of fusion power. In a few years, tritium operation will be extended to higher power. The main role of JET in future years, however, is to provide research support to ITER, for example, in divertor studies.

There are two additional large tokamaks in Europe: Tore Supra in France and ASDEX-U in Germany. Both experiments are in the size class of the U.S. DIII-D. Tore Supra is a circular-cross-section tokamak with superconducting coils. It is capable of 60-second operation — longer than other existing tokamaks but shorter than the planned TPX. Asdex-U is a short-pulse tokamak with moderate shaping, whose research program is focused on divertor and edge physics. About five other mid-size tokamaks are also in operation throughout Europe.

The European program differs from the U.S. program in that it involves a substantial effort in selected alternative concepts, constituting nearly 20% of the total effort. Two substantial alternative concept experiments are under way, namely, a stellarator and an RFP. A medium-scale stellarator, Wendelstein-7AS, is in operation in Germany, a small stellarator is beginning operation in Spain, and a next-step device (Wendelstein-7X) is in the planning stage in Germany. The stellarator is a toroidal concept that is similar to the tokamak, but with the distinguishing feature of being free of plasma current and, therefore, inherently steady state. A large RFP experiment in the DIII-D size class is under way in Italy, and a small RFP is in operation in Sweden. The RFP is a toroidal configuration that is also similar to the tokamak, with the distinguishing feature that the magnetic field required is about 10 times smaller than that of the tokamak. On a smaller scale, low-aspect-ratio tokamaks are under investigation in England. The low-aspect-ratio tokamak is a "fat" tokamak with a small hole in the center of the torus, leading to a more compact reactor.

The European technology development program is active in nearly all of the ITER technology task areas, including superconducting magnets, heating and current drive, blankets, tritium processing, high-heat flux components, materials, remote handling, and safety. A parallel core technology program, of about the same size as the ITER effort, emphasizes longer range research, particularly in the areas of superconducting magnets, blankets, materials, and high-heat flux components. The entire technology program is about twice the size of the U.S. program, although it includes some redundant efforts in the different European countries.

The December 1994 report of the European Union Council of Ministers states that "for the period of 1994 to 1998, the priority objective is to establish the engineering design of the Next Step within the framework of the quadripartite (ITER-EDA) cooperation." Thus, the European Union is supporting the engineering design of ITER, while maintaining "...the option of proceeding towards a European Next Step should cooperation on ITER prove too difficult to continue....". The European Union has not yet made a commitment to participate in the construction of ITER.

The Japanese program is funded at a level about 30% greater than that of the United States (which, as a fraction of GDP represents more than twice the U.S. commitment). It is striking that, 15 years ago, Japan had only a fledgling fusion program; indeed, at that time, Japan was sending teams of physicists to the United States to participate in the forefront experiments. Today, Japan is moving toward a program with two large experiments in the TFTR/JET class. A contemporary of JET, the JT-60 experiment currently in operation is a large tokamak with advanced tokamak features. In addition, a new large stellarator, the Large Helical Device (LHD), is under construction. Its total construction cost is about $1.5 billion, and operation is expected by about 1997. The LHD experiment will investigate stellarator confinement at plasma conditions beyond those obtained in present stellarators. LHD is similar to the new stellarator planned in Germany.

In Japan, fusion R&D is funded by two separate agencies: the Japan Atomic Energy Research Institute (JAERI) and the Ministry of Education (Monbusho). The JT-60 tokamak is funded by JAERI, and the LHD stellarator is funded by Monbusho. LHD is intended to be a national experiment involving researchers from universities across Japan. It will be housed in the new National Institute for Fusion Science. Medium-sized tokamaks and stellarators are also in operation in national laboratories and universities. There are various experiments in alternative concepts, including, in addition to the stellarator, a large tandem mirror and small efforts in the RFP, spheromak, and field-reversed configurations. The latter two belong to a class of configurations called compact tori, which have an aspect ratio of 1 (no hole in the center). More than a third of the total fusion effort in Japan is devoted to alternative concepts.

Japan also has a technology-development program of the same magnitude as that in Europe (almost double that of the United States). The Japanese are active in virtually all areas of ITER technology, and operate a similarly sized core technology program that emphasizes fundamental new technology studies and materials research in universities. Areas of particular strength include advanced superconductors, materials, remote handling, and neutral-beam heating.

The Japanese program is characterized by stability for relatively long periods of time into the future. There is a strong commitment to ITER, and an upgrade of the JT-60 tokamak (called JT-60 Super Upgrade or JT-60 SU) is being considered. JT-60 SU would be a very large steady-state advanced tokamak. Japanese industry is more intimately involved in the fusion program than U.S. industry is in this country's fusion program. Japanese industry plays a central role in the design and construction of fusion experiments, and many industrial scientists and engineers consider themselves to be fusion professionals.

The former Soviet Union was a pioneer in fusion research. Early theory and experiments in Russia led to development of the tokamak. The Soviets developed gyrotrons and were in the forefront of radio-frequency heating of plasmas (now widely used). In recent years, the difficult economic situation has greatly weakened the Russian effort, but medium-sized tokamaks and a stellarator are in operation. Despite the internal difficulties, Russia has contributed its full share of the manpower requirements for the ITER EDA.

2.6 Role of International Cooperation in Fusion R&D

The extraordinary extent of international cooperation in MFE R&D, now extending over nearly four decades, is attributable to a number of factors. Early in the effort, cooperation was facilitated by the recognition that MFE had no significant overlap with the science and technology of thermonuclear weapons, as well as by the recognition that commercial applications, with their attendant competitive pressures, were quite distant in time. Another important factor was the complementarity of different countries' relevant technical strengths at the time: theory in the Soviet Union, diagnostics and engineering in the West.

More recently, the most important driver for maintaining vigorous international cooperation in fusion R&D probably has been the realization that achieving commercialization of fusion is going to be much costlier than was imagined when the effort began. It has become apparent that not only the physics but also the technology of fusion energy pose problems of immense complexity and difficulty. Although some aspects of plasma science and fusion technology can be investigated in experiments of modest scale, it turns out that pushing the frontiers in relation to ignition, steady-state operation, power- and fuel-handling technologies, and materials development requires at least some facilities of very large scale and cost. The corresponding incentives to divide some of these missions among countries — and to build the very largest integrated facilities jointly — are obvious.

The cooperation in fusion R&D in its early years, immediately after the British measurements confirming the performance of the Soviet T-3 tokamak, was extensive but relatively informal. It included exchanges of scientists among the major fusion research centers in the United States, Europe, and the Soviet Union, as well as international conferences to share and compare designs for next-generation experiments. These interactions were instrumental in generating coordination and complementarity among the research objectives pursued in the major facilities of the various countries — visible today as D-T operation in TFTR, noncircular cross section and high current in JET, the divertor-development and impurity-control focuses of JT-60, superconducting coils in T-15, and so on.

As the cooperation continued, many aspects were formalized in negotiated agreements, which currently include the following:

- International Energy Agency (IEA) agreements for cooperation in the medium- and large-scale tokamak experiments, as well as in RFPs and stellarators, in the United States, Europe, and Japan;
- U.S.-EURATOM collaboration on the Tore Supra tokamak in France, the FTU tokamak in Italy, and JET in England;
- an International Atomic Energy Agency agreement for cooperation in fusion-reactor safety studies; and
- bilateral agreements on various aspects of fusion R&D linking the United States with Japan, Russia, Canada, and China.

These collaborations involve, variously, short-term and long-term exchanges of research teams and equipment, coordinated experiments at different facilities, joint planning, and data exchange. Specific examples include:

- installation and operation, by a U.S. team, of a pellet fuel injector at JET; the injector, built at the Oak Ridge National Laboratory in the United States, led to discovery of improved operating regimes for JET;
- financing, by Japan, of the neutral-beam heating system on the Doublet-III machine in San Diego, and training of a Japanese team at the Doublet-III facility preparatory to the start-up of Japan's first large tokamak, the JT-60;
- design of the Japanese LHD stellarator now under construction based on studies conducted at Oak Ridge;
- participation by the European Union and Japan in the Tritium Systems Test Assembly at the Los Alamos National Laboratory; and
- successful completion, under an IEA agreement, of the Large Coil Project, in which six superconducting coils of different designs were built in the United States, Europe, and Japan and then assembled and tested at the Oak Ridge National Laboratory.

The most impressive international collaboration in fusion R&D to date is, of course, the ITER project, the technical features of which have already been described above. The ITER effort is being conducted under formal agreements among the United States, the European Union, Japan, and Russia. In the conceptual-design phase, which ran from January 1988 to December 1990, all four parties contributed personnel and funding to the ITER design team based in Garching, Germany. In the engineering-design phase now under way, a Joint Central Design effort involving multinational teams at each of three sites — Garching, San Diego, and Naka, Japan — is complemented by efforts of Home Teams based on the territories of each of the four partners. About 1200 scientists and engineers are involved in the Joint Central Team and the Home Teams combined. The aim of the engineering-design effort is to produce "a detailed, complete, and fully integrated engineering design of ITER and all technical data necessary for future decisions on the construction of ITER." Proceeding with construction is not a foregone conclusion, however, and will require a further formal agreement that is to be negotiated on completion of the design; issues to be settled include which country will host the actual facility and how the costs of construction will be divided among the host country and the other partners.

2.7 The Pace of Progress in Fusion R&D

Progress in fusion can be gauged in two ways, both of which demonstrate the remarkable advances that have taken place in fusion research over the past decade. The first measure of progress involves the key figures of merit relating to plasma quality, such as density (n), temperature (T), energy confinement time (τ), the triple product of these parameters ($nT\tau$), the fusion power (P_f), and the fusion-power amplification factor ($Q = P_f$ divided by the input power to the plasma). These plasma-quality factors reflect both the state of understanding in plasma physics and the state of development of plasma technologies (magnets, plasma heating and fueling systems, and so on) needed to translate physics understanding into operational plasmas.

All of the plasma-quality indicators — including, above all, the integrating, "bottom line" indicators of fusion power and amplification factor — have increased over the years at rates that have been extraordinary in steadiness and magnitude. The increase in fusion power over the past 20 years has been a factor of 100 million, from 0.1 watt in 1975 to more than 10 million watts in 1995. (This progression is shown in Figure 2.) The amplification factor, Q, has increased similarly, to a point now very close to the "break-even" value of Q=1. (JET has obtained a projected Q value — that which would have been obtained if the fuel had been D-T — of 0.7) The rapid and steady growth of these performance parameters over time compares favorably with the growth rates exhibited by the performance of computer chips in the same time period.

The second measure of progress is the extent of the qualitative developments in plasma physics that, in fact, have underpinned the progress in the quantitative indicators just described. Illustrative of these qualitative developments and their impact are the improvements that have taken place in understanding of plasma pressure limits, of the self-generated "bootstrap" currents that occur in tokamaks, and of the H-mode of tokamak plasma operation.

- If a fusion reactor is to have suitably high power density, the plasma pressure (the product of density and temperature) must be high. Plasmas tend to become unstable beyond a threshold in pressure, however. Above this pressure limit, large waves develop that throw the plasma against the surrounding vessel, which cools the plasma and quenches the fusion reaction. A relatively complete theory of such processes, which are called macroscopic instabilities, has been formulated within the framework of magnetohydrodynamics (MHD), a construct in which plasma is analyzed as a conducting fluid. The theory is able to predict the pressure limit and the dynamics of the instability, and has been confirmed in numerous experiments. In combination with large-scale computation, moreover, the theory can be used to modify tokamak design to increase the pressure limit, as well as to identify alternative configurations that might outperform tokamaks. Current advanced tokamak concepts arose from the use of this theory. It has also proven to be of great usefulness in space physics.

- Some 20 years ago, it was predicted that a tokamak plasma would spontaneously generate its own current, even in the absence of an electric field. This "bootstrap" current is driven by pressure gradients within the plasma, a subtle result of the complex particle orbits within a tokamak. It can reduce the necessity for external current drive systems, thereby simplifying and reducing the cost of a tokamak reactor. Long considered only a theoretical hope, or "too good to be true," the phenomenon now has been confirmed in a series of experiments, showing that millions of amps of plasma current can be sustained by plasma pressure gradients. Bootstrap currents, at the forefront of theoretical plasma physics just 10 years ago, today are used routinely by engineers designing improved reactor configurations.

- Some 10 years ago, it was discovered empirically that, under certain conditions the energy confinement time in tokamaks roughly doubled from its expected value. This H-mode of tokamak behavior (shorthand for high-energy-confinement mode) has since been observed in many different experiments, strongly indicating that the phenomenon is not an artifact of a particular device. In contrast to the bootstrap current story, where theory led to experiments that confirmed the result, in the H-mode case an experimental result led to development of a line of theoretical research. The still unfolding H-mode theory has shown that the plasma turbulence responsible for energy leakage from the plasma is reduced in the H-mode by strongly sheared rotational flow of plasma near the plasma edge. This theory is at the forefront of turbulence research, a notoriously challenging area in 20th century physics. It has, nonetheless, led already to practical techniques for controlling plasma turbulence to improve energy confinement.

These examples, which illustrate — although not exhaustively — the striking progress in understanding fusion plasmas, are all classic cases of the interaction of theory and experiment through which science normally progresses. In combination with other advances, these improvements in understanding have been responsible for the 10-million-fold improvement in Q and the 100-million-fold increase in fusion power achieved in MFE research during the past 25 years. Only a rather modest further extension of this progress is now required to reach the performance range needed for ignition.

Chapter 3 EVALUATION OF THE CURRENT U.S. PROGRAM AND PLAN

The reasonableness of a program of R&D can be judged on both "internal" and "external" criteria:

- The internal criteria have to do with whether the goal of the program is reasonable; with whether the program elements likely to be necessary to achieve the goal are all present, including redundancy appropriate to the level of uncertainty about the outcomes of particular elements and to the urgency of the goal; with whether the individual elements are suitably designed for their purposes; with whether they are suitably linked and phased; and with whether they are being funded at levels commensurate with the tasks involved, the opportunities available, the other (competing or cooperating) entities pursuing or likely to pursue the same goal, and the timing desired.

- The external criteria have to do with whether the funding allocated to the program can be justified in relation to the total resources available in the agency, or in the society, for similar activities, e.g., for all energy R&D or for all R&D in general; judgments in this category entail reaching conclusions about the importance and prospects of attainment of a program's goals in relation to its cost and in relation to the importance/prospects/cost combinations of other R&D programs.

Our conclusions here about the reasonableness of the U.S. fusion energy R&D program are necessarily confined mainly to the internal criteria. We had neither the mandate nor the resources nor the time to compare fusion R&D with other areas of energy R&D, not to mention with other areas of research such as, for example, high-energy physics, space, or global environmental change.

We do assert, relevant to the external criteria, that the absolute importance of having economically affordable, environmentally tolerable, and politically acceptable energy sources adequate to meet society's energy needs in the middle of the next century is high enough to warrant substantial R&D investments to increase the likelihood of this outcome, and that the arguments are compelling for attempting to develop all of the major, long-term energy options to the point that their potential to contribute can be confidently assessed. We also assert that, in light of the observation that successful high-technology enterprises typically invest 2-3 percent of gross revenues in R&D, and in light of U.S. gross electricity revenues' running currently in the range of $200 billion per year, it would be perfectly reasonable for U.S. electricity-supply R&D to be spending $4-6 billion per year. If half of that were spent on long-term options and a third of the half were spent on fusion, the result would be national fusion-energy R&D expenditures of $0.7-1 billion per year. (This is without reference to the non-energy benefits of fusion R&D, such as advances in basic science and spinoffs into technologies for non-energy applications.)

As for our evaluation of the reasonableness of the DOE's program of fusion-energy R&D on the "internal" criteria, which is an important part of our charge, we have been able to draw upon a number of major reviews of the program that have been conducted by others in the period since 1989[1]; on analyses conducted for and embodied in the Energy Policy Act of 1992 (Public Law 102-486, 24 October 1992); and on an extensive series of briefings to our Panel, during the two and a half months of our study, by participants in all of the main elements of the U.S. program and by those responsible for overseeing it (in DOE) and for reviewing its budget (in the Office of Management and Budget).[2] Our conclusions about the reasonableness of the program on the "internal" criteria, based on these inputs and our own deliberations, follow.

3.1 Goal of the Program

The National Energy Strategy (NES) formulated during the last 2 years of the Bush Administration called for a vigorous program of fusion energy R&D aimed at operation of a demonstration reactor by about 2025 and operation of commercial fusion power plants by about 2040. This timetable has been taken seriously by DOE, and it has been the basis for DOE's choices about how to shape the program and for its plans and expectations about budgets.

Is it a reasonable goal? For an affirmative answer, the goal should be both desirable and potentially attainable. In view of the importance of having fusion available as a commercial energy source by the middle of the next century (Chapter 1), we believe the goal is clearly desirable. In view of the pace of progress in plasma physics and fusion technology over the past two decades (Chapter 2), we also believe there would be a reasonable probability of attaining the goal — that is, of meeting the NES timetable or something close to it — assuming that the requisite R&D funds were available.

The conclusion that trying to harness fusion energy on this time scale is reasonable is supported explicitly or implicitly in all of the recent reviews, as well as having been repeatedly reaffirmed by the U.S. Congress. Acceptance of this goal was also nearly unanimous among our briefers.

3.2 Presence of Needed Elements

The previous reviews and our own briefers were also in striking agreement on what elements need to be present in a sensible program to achieve the indicated goal: a strong and broad "core program" of theory and experiment in plasma science and fusion technology, plus reactor-systems studies linking these ingredients; early demonstration of ignition and reasonably prolonged burn of a fusion plasma; investigation and demonstration of the application of advances in confinement physics needed for steady-state operation and for the design of more compact, more economical reactors; development and demonstration, at near-commercial scale, of the fuel-management and power-handling technologies needed to turn a fusion plasma into an electric power source; exploration, at a lower level of effort, of alternative confinement concepts that may be worthy of further development if the mainline (tokamak) concept falls short; development of advanced fusion-reactor materials that can reduce radioactivity burdens and enhance reactor performance; increased engagement of industry in the fusion development effort; and strong international cooperation to exploit diverse capabilities and share costs.

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[1] These reviews include the late 1989 report of the Committee on Magnetic Fusion in Energy Policy of the Energy Engineering Board of the National Research Council (NRC 1989), the September 1990 report of the Fusion Policy Advisory Committee to the Secretary of Energy (FPAC 1990), the September 1991 report of the Fusion Task Force of the Secretary of Energy Advisory Board (SEAB 1991), the September 1992 report of the Fusion Energy Advisory Committee to the Secretary of Energy (FEAC 1992), and the February 1995 report on the fusion energy program by the Office of Technology Assessment of the U.S. Congress (OTA 1995).

[2] The briefings received by the Panel are listed in an Appendix to this report.

Concerning the actual presence of these elements in the U.S. program, it is clear from the discussion in Chapter 2 that:

- ITER is to provide the ignition demonstration together with a platform for development and demonstration of relevant fuel-management and power-handing technologies, in an international context; and

- TPX and, to a lesser extent, continued operation of the medium-sized tokamaks at GA and MIT are to provide the U.S. contribution to exploring applications of advanced confinement physics and the attainment of steady-state tokamak operation.

In addition, there is widespread agreement among the previous reviews and our briefers — and we also agree — that

- although a strong core program has existed and continues to exist within the U.S. effort, it is not as broad or as strong as is desirable, it is focusing increasingly on support of the ITER and TPX projects, and it is in danger of being squeezed down to inadequacy by the drain on the budget generated by these projects as they enter their construction phases;

- the effort on alternative concepts, which was practically eliminated in the series of project cancellations brought on by program budget cuts through the 1980s and into the early 1990s, is now wholly inadequate;

- the effort on advanced materials, although singled out as a priority by virtually every review, has never received the resources it requires and deserves — and might well not receive them in the future, even under the overall budget increases reflected in current DOE planning;

- the engagement of industry in the U.S. fusion effort has been growing, but needs to be further strengthened.

The necessary ingredients, then, although all are present, are not all adequate, and redundancy is weak. We elaborate on these points in the element-by-element evaluation of the program that follows.

3.3 Evaluation of Individual Elements

In what follows, we look more closely at the core program, TFTR, ITER, and TPX, in turn.

Core Program

The core program, which was described in some detail in Chapter 2, consists of work in tokamak improvements, alternative concepts, basic theory and computation, basic experimental studies, and fusion-technology research (including materials development). As noted in Chapter 2, the combination of a declining budget (relative to a decade ago) and increasing funds allocated to design of ITER and TPX has yielded a core program only a fraction of its former size. Its diminution has produced a widespread concern in the fusion community that the program has become dangerously out of balance — too large a sum is being gambled on future large experiments which may not even be built, at the expense of the essential core program of active research. Particularly troublesome constraints are being felt in alternative concepts, basic theory and computation, basic experiments, and materials development. These constraints clearly threaten progress in fusion.

The most glaring victim of the decline in emphasis on the core program has been research on alternative concepts. The disappearance of most of the alternative-concept work from the U.S. program over the past decade is chronicled in Table 2. Although we fully understand the character of the budgetary pressures that contributed to this result, we believe that the tiny fraction of U.S. fusion R&D funds currently being devoted to alternative concepts is wholly inadequate. It is clear to us that interesting ideas exist at present that deserve experimentation. Many of these ideas can be advanced, moreover, in small- and medium-sized experiments; they are not at the point in the development trajectory characterizing the tokamak, where large facilities are needed to gain increased understanding. We think scientific common sense requires sustaining a vigorous program in alternative concepts. Other recent reviews of the fusion program have consistently made the same point.

The only alternative concept of any real vitality in the U.S. fusion program at present is the nonmagnetic alternative — namely, the work on inertial-confinement fusion that has progressed not because of support from the fusion-energy program but because of support in the defense budget to pursue insights about nuclear weaponry through inertial confinement research (see Section 2.3). The small allocation for IFE studies in the current MFE budget is intended to explore aspects of IFE that would be important in reactor applications but are not being pursued in the defense-funded inertial confinement research. It is not clear that the amount is adequate even for that limited purpose.

TFTR

Princeton's TFTR is the lead tokamak in the United States and a peer of the JET tokamak in Europe and the JT-60 tokamak in Japan. It began operation in December 1982, with plans for two phases of operation: a deuterium phase to study the plasma physics of a large tokamak and a D-T phase to demonstrate fusion energy production on a pulsed basis. As indicated briefly in Chapter 2, TFTR has been a highly successful experiment: All of the objectives set for it prior to its operation have been met or exceeded within the anticipated budget, although the schedule has been constrained by annual funding rates.

Table 2. Fate of Alternative MFE Concepts in the United States Since 1985[a]

Concept	Facility/ Location[b]	Construction Cost (million $)	Fate of Project
Mirror machine	MFTF-B/ LLNL	372	Closed in 1986 upon completion of construction.
Stellarator	ATF/ ORNL	19	Operated intermittently since opening in 1990. mothballed 1994.
Field-reversed configuration	LSX/ U of WA	14	Operated minimally upon completion in 1992; being relocated for use in tokamak fueling experiments.
Reversed-field pinch	CPRF/ LANL	58[c]	Cancelled during construction. 1992.
Reversed-field pinch	MST/ U of WI	4	Operated at reduced budget since opening in 1988.
Spheromak	MS/ U of MD	4	Maryland spheromak phased out in 1992 without attaining expected performance.
Spheromak	S-1/ PPPL	9	Constructed in 1983. operated until 1987.

[a] Source: OTA (1995). Table 4-4.

[b] Abbreviations: MFTF-B/LLNL. Mirror Fusion Test Facility. B. Lawrence Livermore National Laboratory: ATF/ORNL. Advanced Toroidal Facility. Oak Ridge National Laboratory: LSX/U of WA. Large S Experiment. University of Washington: CPRF/LANL. Confinement Physics Research Facility. Los Alamos National Laboratory: MST/U of WI. Madison Symmetric Torus. University of Wisconsin: MS/U of MD. Maryland Spheromak. University of Maryland: S-1/PPPL. Spheromak-1. Princeton Plasma Physics Laboratory.

[c] Unfinished

In its deuterium phase, TFTR achieved a record ion temperature of nearly 500 million degrees C, provided the first experimental demonstration of the bootstrap current in a tokamak, and demonstrated how to attain very high plasma pressures using current-profile control. In its D-T phase, TFTR attained $P_f = 10.7$ megawatts and $Q = 0.3$, as mentioned above, as well as producing a range of alpha-particle physics results of great interest.[3] The TFTR D-T experiments, which involve extensive tritium handling, have been performed with an excellent safety record — a significant engineering and management success. Although TFTR is scheduled to shut down at the end of FY1995, the TFTR group has developed plausible plans for operating it longer to extract additional benefit from its unique capabilities, if funds are available.

ITER

Previous reviews of the U.S. program in general — and of ITER in particular — have emphasized, and we agree, that

- ITER's primary mission of ignition and sustained burn, and its secondary mission of utilizing its burning plasma as the core of a test-bed for development and testing of key components of fusion-reactor technology, are essential milestones on the path toward a demonstration reactor;

- the current design of ITER represents an intelligent approach to accomplishing the primary and secondary missions with high confidence; that it is a large, complex, costly tokamak based on relatively conservative confinement physics was the natural result of the desire of all of the partners (i) to cover enough ground with ITER to make design and construction of a demo possible at the next step (thus making it conceivable that a demo could operate by 2025 or so) while (ii) minimizing the chance that ITER would fail to ignite (which, obviously, would be a huge setback for progress toward fusion energy);

- given the magnitude of this project, international cooperation in financing it and carrying it out not only makes excellent sense, it is practically a necessity; and

- the international cooperation on ITER so far, through the conceptual-design phase and well into the engineering-design phase, has been very successful (notwithstanding the predictable difficulties of conducting international negotiations about every step) not only in cost-sharing but also in building highly effective multinational technical collaborations; this success has made the ITER project a highly visible example of the potential for international cooperation on complex and costly science-and-technology projects.

At the same time, certain reservations about the ITER project have emerged from prior reviews and, as evidenced by the briefings and letters provided to our Panel by the U.S. fusion community, are being felt with increasing urgency at a time of severe constraints on funding for R&D in general and fusion in particular. Specifically:

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[3] First-time measurements have been performed on alpha-particle slowing down, alpha-particle confinement, and a range of other issues relevant to the influence of alpha particles on plasma behavior and the prospects of ignition. (See also the subsection on ignition in Chapter 2.)

- even divided four ways (among the United States, the European Union, Japan, and Russia), and even before the construction phase begins, the costs of ITER have imposed a significant drain on the U.S. fusion budget, severely constraining the availability of funds for the existing major U.S. tokamak experiments, for alternative concepts, and for other ingredients of the core program including plasma theory and materials development;

- the higher budgets that must follow if ITER is to proceed to the construction phase will be even more problematic from the standpoint of the ability of the United States to maintain needed depth and diversity in its domestic program of fusion research; and a science-and-technology project this costly — $10-13 billion for design, construction, and operation — is vulnerable to budgetary pressures arising in ANY of the partners; thus ITER may amount to putting too many of fusion's eggs in a very fragile international basket;

- by relying on a conservative approach to confinement physics, ITER will (at best) end up demonstrating an approach to fusion that is unlikely to be extrapolatable to an attractive commercial reactor — the power density in this approach would be low, the physical size of the plant and hence the cost of construction and maintenance would be high, and the minimum practical unit size would be so large as to restrict interest in such plants to only the largest electric utilities; a demonstration of this approach, even if completely successful in physics and technology-development terms, could be a setback for fusion's prospects of commercialization unless accompanied by parallel demonstrations of approaches likely to lead to more compact, more economical reactors.

Our own synthesis of the attractions and liabilities of the ITER project is that

- ITER as currently construed is a reasonable approach to achieving, in one device and on a time scale commensurate with operation of a demo around 2025, the key ignition, prolonged-burn, and technology-development steps essential to such a timetable;

- in order to avoid both the appearance and the possible reality of locking fusion development onto a pathway toward a suboptimal reactor, the currently programmed ITER would need to be accompanied by other domestic or international projects to demonstrate directions of tokamak evolution, or development of alternative approaches, that are more likely than ITER *per se* to lead to an attractive reactor; and

- if U.S. (and/or other) fusion R&D budgets are not likely to permit pursuing the multi-pronged approach just described, then ITER and the rest of the international fusion collaboration will need to be somewhat restructured; a set of recommendations for how to proceed in this direction, in a budget-constrained case, is presented in Chapter 4.

TPX

TPX, as discussed in Chapter 2, would be a national facility located at Princeton and would study advanced tokamak plasmas under steady-state conditions. The origin of the project can be traced to the 1991 recommendation of a Secretary of Energy Advisory Board (SEAB) Task Force to cancel BPX, then under design, for fiscal reasons. The SEAB Task Force recommended that a less costly machine — in the $0.5 billion range — be developed instead, and the TPX focus and design then resulted from a national process established to determine the best way to proceed within the indicated fiscal constraint.

The TPX steady-state experiment is complementary to ITER (or any other ignition experiment): As indicated in the discussions of ignition and steady-state operation in Chapter 2, both missions are critical to achieving practical fusion energy. An experiment of either type can be justified either in the presence or in the absence of the other. Tokamak physics is sufficiently advanced to permit construction of an ignition machine with a high probability of success; but attainment of ignition will not, in itself, establish that commercially attractive fusion reactors are feasible. This demonstration may well depend on the sorts of advanced tokamak physics that TPX has been designed to test and that offer the possibility of more compact, more economical fusion reactors.

TPX is now ready to begin construction. The design produced by the nationally constituted project has been extensively and favorably reviewed; it appears to do an excellent job of reconciling physics goals and engineering constraints, and would lead to a machine costing about $750 million. The degree of industrial involvement in the project that has been envisioned would be a significant benefit, providing industry with experience in fusion technology ranging from construction of superconducting magnets to systems integration. We believe that proceeding with TPX would be highly desirable, given funding for U.S. fusion R&D in the range planned for by DOE in connection with the 2025 demonstration-reactor goal.

3.4 Linking and Phasing of Program Elements

The elements of the current U.S. program are, for the most part, complementary and well coordinated with each other, with fusion R&D in other countries, and with the international collaborative effort in fusion energy. This positive situation is the result of several factors: the high degree of communication and interaction within the U.S. fusion community and between it and the world fusion community; the conscious decisions made by the program's managers in the DOE, and their counterparts in other countries, to devote different machines to different key tasks in fusion development, avoiding excessive duplication of effort; the exceptional frequency and diligence of the high-level reviews of the direction and priorities of the U.S. fusion energy R&D program (possibly the most reviewed science and technology program in history) and in the ITER project; and, less positively, the overwhelming dominance of the tokamak line of development within the U.S. program, which has ensured that even most of the basic theory and basic experimental work in the core program relate to tokamaks. Most of the specifics that add up to this high degree of coordination and complementarity have already been described in earlier sections.

Probably the strongest criticism that can be leveled at the U.S. and world programs with respect to the phasing of their elements relates to the argument, mentioned above, that the ITER experiment in ignition, sustained burn, and associated technology testing might benefit greatly from the kinds of developments only now emerging in advanced tokamak physics. From this standpoint, it might have been better if ITER had come later, in that physics advances being demonstrated in current-generation and next-generation machines at a scale smaller than ITER could have allowed the ITER goals to be attained in a more compact, less costly device that better pointed the way to an attractive reactor. It can be argued on the other side, however, that the timing of ITER has been dictated by the desire to operate a demonstration reactor by about 2025 and, perhaps even more relevant, the desire to achieve ignition and sustained burn within the lifetimes of some of the scientists whose careers have been devoted to fusion energy and some the legislators voting for fusion budgets.

A somewhat related criticism of the phasing of program elements is that development of advanced materials for fusion reactors has not proceeded at a sufficient pace to enable ITER to employ such materials in its principal components (although it may be used in its technology-testing phase to test components made of such materials). Materials research, despite much lip service, simply has not yet been given the priority or the funding that will be required for it to affect choices for next-generation machines. So far this is really more a problem of omission than of phasing.

3.5 Adequacy of Funding

The last decade's decline in U.S. government spending on fusion R&D was not the result of diminished capacity to spend the money effectively, or of diminished prospects for success in reaching the energy goal, or of informed conviction that other energy sources will suffice to meet the energy needs of the next century in economically affordable, environmentally tolerable, and politically acceptable ways. Quite the contrary, by all of these indicators spending on fusion should have increased, as it has in Europe and Japan. Rather, the U.S. decline was a consequence of pressures on the Federal budget generally, and on energy R&D budgets particularly, occasioned in part by the belief that much of what the Federal government has been doing could be done more efficiently by state and local governments or by the private sector.

In the case of fusion, however, any expectation that a substantial part of the funding for current and near-term-future fusion energy R&D will come from the private sector is not realistic. The investments are too large, and the possibility of economic returns is too distant, to elicit significant funding from the private sector at this stage of fusion-energy development. Because fusion is not of particularly greater relevance to one state or region than to another, and also because of the size of the investments required, it is not realistic to expect significant investment in fusion energy R&D from states, either. Indeed, the investments required to pursue fusion energy development to its logical next steps are so large as to threaten to exceed what even the richest industrial nations are prepared to invest in individual projects; as noted above, this situation constitutes one of the primary incentives for international collaboration on these steps. Thus, the characteristics of fusion energy R&D — very large potential benefits for society as a whole, large investments required to secure these benefits, commercial application decades away

— constitute a classic case for bearing the funding burden at the level of the Federal government and, indeed, at the level of a consortium of governments.

It is important to understand that the relatively high cost of fusion energy R&D is not a consequence of profligacy on the part of fusion researchers or mismanagement on the part of DOE. It is a consequence of inherent properties of the most promising approaches to harnessing fusion that have been discovered so far, for which the energetics and scaling are such that ignition can only be approached, attained, and studied in devices of great size and technological sophistication. This is in sharp contrast with many other branches of energy research, which can be productively pursued at much smaller scales and lower costs.[4] That does not mean, however, that only these other energy options, and not fusion, deserve to be pursued. We have argued in Chapter 1 that civilization is likely to need, in the middle of the next century and beyond, all of the safe, affordable, environmentally tolerable, politically acceptable energy it can get. If some of the options that can meet these criteria are costlier to develop than others, they nonetheless *should* be developed as long as their prospective benefits are much higher than their development costs.[5] We believe that fusion R&D meets this test and that, therefore, prior decisions to provide substantial funding for fusion R&D were not wrong.

The funding provided, in fact, has generally not been as great as the major reviews of the program have recommended. For example:

- A National Research Council review of the MFE program completed in 1989 concluded that funding would have to be increased by 20% each year in the early 1990s and by larger amounts in the late 1990s to allow the United States to proceed with CIT and with ITER construction, and it recommended that this be done (NRC 1989).
- The Fusion Policy Advisory Committee of the Energy Research Advisory Board concluded in 1990 that appropriate progress of the MFE program toward the objective of a demonstration reactor by 2025 would require increasing the budget from $316 million in FY1990 to more than $600 million (FY1990 dollars) in FY1996; even FPAC's "constrained" program called for the MFE budget to increase to $470 million (FY1990 dollars) by FY1996 (FPAC 1990).
- Even after the cancellation of the BPX (into which CIT had evolved), the SEAB Task Force on Energy Research Priorities concluded in September 1991 that "The Task Force

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[4] We note also that this property of fusion science — the requirement for devices of great size, sophistication, and cost in order to make major advances — is *not* in contrast with, but in fact very much resembles, the situation in other branches of science (such as nuclear physics and high-energy physics) with which plasma physics could be considered comparable in importance *as* science, irrespective of its energy applications.

[5] One member of the Panel (not from the fusion community) formulated this proposition, in the course of our discussions, in a way that bears repeating here: If you are offered three investments, each with a net present expected value of $100, and two of the investments cost $1 while the third costs $10, the correct strategy is not to make just the first two investments but to make all three (assuming that there are no other $100 gains to be had at the $1 price, and assuming that the difference between ending up with $200 and ending up with $300 is important to you).

believes that funding for the magnetic fusion program must increase at a modest rate (e.g., 5% real growth per year)...". (SEAB 1991).

- The Fusion Energy Advisory Committee for DOE's Office of Fusion Energy concluded in September 1992 that MFE budgets would have to increase by at least 5% per year in real terms over the FY1993 level of $331 million, reaching $420 million (in FY1993 dollars) in FY1998, with a further increment thereafter for ITER construction, to be plausibly consistent with the 2025 target date for operation of a demonstration reactor (FEAC 1992).

None of these recommended increases materialized. The U.S. MFE budget has remained essentially flat in real terms since FY1990. No one has relieved DOE's Office of Fusion Energy of the burden of pursuing the NES goal of a demonstration reactor in 2025, but neither has the government seen fit to provide the money that every review has concluded would be needed to meet that goal.

Accordingly, the OFE has continued to develop program plans, and corresponding budget projections, that are plausibly consistent with the NES timetable. As we noted above, the current OFE program plan calls for a budget that rises steadily from $366 million in FY1996 to $860 million in FY2002 before falling to about $700 million in FY2005 (all as-spent dollars). The average for the 10-year period from FY1996 to FY2005 would be $645 million per year. We believe these budgets are reasonable. Indeed, they are the minimum amounts required to support full U.S. participation in the construction phase of ITER as currently envisioned while maintaining a vigorous, complementary domestic program that (a) extracts the remaining scientific value from experimental facilities already in operation; (b) constructs the TPX to explore crucial issues not accessible in existing devices or in ITER, and to anchor the domestic experimental program in the next century; and (c) nourishes essential efforts in smaller experiments (including alternative concepts), theory, computing, technology development, and fusion-reactor materials. It almost certainly would not be possible to spend less and still meet the NES fusion timetable. The indicated amount also can hardly be said to be beyond the financial means of the United States; $645 million per year could be raised with a 0.3% tax on current U.S. electricity sales.

None of this changes the political reality of the current budget-cutting climate, however, and that reality indicates that fusion funding at levels even approaching those in the OFE's current plan will not be forthcoming. We devote most of the next chapter, therefore, to an analysis of how the most important priorities within fusion energy R&D could be preserved at lower budget levels.

Chapter 4 RECAPITULATION, STRATEGY, AND RECOMMENDED PROGRAM

In this chapter, we recapitulate the main findings from Chapters 1-3, describe the principles and priorities appropriate to managing the U.S. fusion energy R&D program in a budget-constrained environment, and present and explain our recommendations about specific elements of a suitable budget-constrained program.

4.1 Recapitulation

Funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond. This funding also sustains an important field of scientific research — plasma science — in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable in other fields of science and in industry. And U.S. funding has been crucial to a productive, equitable, and durable international collaboration in fusion science and technology that represents the most important instance of international scientific cooperation in history as well as the best hope for timely commercialization of fusion energy at affordable cost.

World energy demand in 2050 is likely to be at least twice as large as it was in 1990, electricity demand at least three times as large as in 1990. Failure to make sufficient electricity and other energy forms available in safe, economically affordable, environmentally tolerable, and politically acceptable ways will be a prescription for widespread frustration of economic aspirations, accompanied by social tensions, political instability, and environmental deterioration. Other than nuclear fusion, the options potentially available for meeting the energy and electricity demands of the next century are fossil fuels, renewable energy sources, and nuclear fission. Each of these options is likely to be playing a role in world energy supply in 2050 and beyond; each has the potential for improvement over its technical, economic, and environmental characteristics of today, and deserves investment to achieve this potential; and each, unfortunately, is subject to shortcomings and constraints that could limit its contribution.

Fusion energy offers (a) fuel extractable from ordinary seawater (thus available to all countries) and sufficient in quantity for millions to billions of years; (b) significant advantages over fission-energy options with respect to possibilities of minimizing radiological hazards and links to nuclear weaponry, over fossil fuel options with respect to emissions to the atmosphere, and over many forms of renewable energy with respect to impacts on ecological and geophysical processes; and (c) the prospect of monetary costs comparable to those of other medium-term and long-term energy options. Diversity in the menu of energy options for the future is essential. There are not so many possibilities altogether. The more of these that can be made usable, the greater will be the chance that overall energy needs can be met without encountering excessive costs from or unmanageable burdens on any one source. The potential value of developing fusion energy should be understood in this context. The possible costs of needing fusion at midcentury and beyond, but not having it, are very high.

Progress in fusion energy R&D has been enormous over the past two decades by almost any standard, but there is still far to go. The physics and engineering of fusion are complicated, and, although aspects of plasma science and fusion technology can be demonstrated at small to modest scale, the inherent properties of the most promising approaches to harnessing fusion that have been discovered so far are such that taking the next logical steps requires devices of great size and technological sophistication. The high costs of such steps, combined with the uncertainty of success and the long time scale before there is the possibility of a return on the investment, explain why governments, and not the private sector, must bear the main burden of funding fusion R&D at this stage of the technology's development. Indeed, the costs of the next steps are high enough to constitute a considerable incentive for governments to join forces in international collaborations toward these ends.

Based on the importance of developing energy sources adequate to meet the needs of the next century and the promise of fusion for this purpose, the benefits of fusion R&D in strengthening the national science and technology base, the impressive recent rates of progress in fusion research, the costs of the logical next steps, and the growing investments being made in fusion R&D in the European Union and Japan (which already total more than three times the corresponding investment here), we believe there is a strong case for the funding levels for fusion currently proposed by DOE — increasing from $366 million in FY1996 to about $860 million in FY2002 and averaging $645 million between FY1995 and FY2005 (all in as-spent dollars). Spending less would drastically reduce the chance of meeting the National Energy Strategy goal of operating a fusion demonstration reactor by about 2025.

4.2 A Budget-Constrained Strategy for Fusion R&D

Although DOE's program plan and associated budgets are reasonable and desirable, they do not appear to be realistic in the current climate of budgetary constraints. We therefore have devoted most of our effort in this study to developing a budget-constrained U.S. fusion R&D strategy that, given level funding at about half of the average projected for the period FY1996 through FY2005 under the current DOE plan, would preserve what we believe to be the most indispensable elements of the U.S. fusion effort and associated international collaboration. This strategy would cost about $320 million per year, $46 million less than the U.S. fusion R&D budget in FY1995.

Embracing this strategy would entail hard choices and considerable pain, including straining the patience of this country's collaborators in the international component of the fusion effort, forcing difficult trade-offs between even a reduced U.S. contribution to international collaboration and maintaining adequate strength in the domestic components of U.S. fusion R&D, shrinking the opportunities for involvement of U.S. industry in fusion technology development, and surrendering any realistic possibility of operating a demonstration fusion reactor by 2025. But we believe it is the best that can be done within budgets likely to be sustainable in the current climate, and the least that can responsibly be done to maintain a modicum of momentum toward the goal of practical fusion energy. The rest of this chapter is devoted to describing and explaining this budget-constrained overall strategy and our specific recommendations for implementing it.

The strategy we recommend is aimed at ensuring maximum benefit from the investment of public funds, at promoting a logical progression of steps directed toward the ultimate goal of commercialization of fusion energy, and at sustaining a significant and stable scientific infrastructure (people and facilities) capable of implementing our program and of capitalizing on future opportunities. Consistent with these aims, we believe such a strategy must:

- ensure the vitality of the plasma-science and fusion-technology communities (including universities, national laboratories, and industry);
- use and leverage the facilities and nuclear expertise that exist in all three of these sectors;
- expand the partnerships among these sectors;
- conduct the program in a way that ensures U.S. industrial capability and competitiveness;
- emphasize international cooperation and joint international projects.

Combining these objectives with consideration of the possibilities presented by the current state of development of fusion energy and by the existing elements in the fusion R&D programs of the United States and other countries has led us to conclude that the key priorities of a budget-constrained U.S. program should be as follows:

- a strong domestic core program in plasma science and fusion technology, with funds to explore both advanced tokamak research and research on concepts alternative to the tokamak, leveraged where possible on related activities worldwide;
- a collaboratively funded international fusion experiment focused on the key next-step scientific issue of ignition and moderately sustained (circa 100 seconds) burn, at a cost about one-third that of ITER as currently planned; and
- an international program to develop practical low-activation fusion-reactor materials, highly desirable for economical reactor performance and environmental attractiveness.

Pursuit of improved understanding of advanced tokamak physics, including steady-state operation, in TPX is also a very important goal. For reasons given below, however, we believe that, in a budget-constrained program it must be assigned a lower priority than the preceding elements.

4.3 Specific Recommendations, With Explanations

The recommendations and findings that follow represent the Panel's considered, unanimous judgment about how this country's fusion energy R&D program, in a time of great fiscal stringency, can most effectively make progress toward the goal of practical fusion energy, sustain the important field of scientific research that plasma science has become, and maximize the efficiency with which the public's funds are used. In what follows, our recommendations and findings are presented in **boldface** type, with wording identical to that in the Executive Summary, and explanations and elaborations of the recommendations and findings are interspersed in *italic* type.

This budget-constrained, internationally integrated U.S. fusion R&D program would, more specifically:

- **preserve and somewhat enhance the U.S. core program in relation to its FY1995 level of about $180 million per year, including a degree of remedy of the current program's neglect of confinement concepts other than the tokamak;**

- **continue to operate, within the core program, the medium-scale tokamaks at General Atomics (DIII-D) and MIT (Alcator C-MOD), upgrading DIII-D after Princeton's Tokamak Fusion Test Reactor (TFTR) is shut down, and continue modest funding to pursue energy applications of the inertial-confinement fusion effort being funded for stockpile stewardship purposes in the weapons budget;**

The core program includes small- and medium-scale plasma-confinement experiments (varying in construction cost from under $1 million to the range of $200 million), research investigating improvements to the tokamak concept and concepts alternative to the tokamak, basic fusion technology (including materials development), and plasma theory. Chapters 2 and 3 have described the content of these elements of the core program, their critical importance to the viability of the U.S. fusion-energy R&D effort and to the prospects for attaining fusion's highest potential as a commercial energy source, and the constraints they have increasingly suffered as a result of budgetary stringency and rising expenditures on the big devices outside the core program. We believe that a vigorous core program, containing all of the elements just mentioned, must be preserved whatever the overall fusion-energy R&D budget. In the budget-constrained scenario with fusion-energy R&D spending level at $320 million per year (as-spent dollars), we think the core program could and should be funded initially at its FY1995 level of about $180 million per year, with modest increases possible over the 10-year period we have considered. A stable or slowly growing core program would, of course, entail the continual termination of experiments as they complete their useful lives and the continual start-up of new experiments, as needed to address current issues.

As noted in Chapters 2 and 3, alternative confinement concepts constitute an element of the core program that has been particularly badly constricted by the way DOE's program has evolved over the past decade. Alternative concepts have a number of features that might lead to marked improvements in the attractiveness of fusion as an energy source; at their current state of development, moreover, a number of concepts that deserve further investigation can be pursued at relatively modest cost. The Panel recommends that research on alternative concepts be expanded, even in the context of a level or only slowly rising core program. This effort should include (but not be limited to) support for exploring the energy applications of inertial-confinement fusion, especially the development of the heavy-ion drivers likely to be needed for energy applications but not being funded in the defense programs that support most of this country's inertial-confinement fusion research.

Notwithstanding the need to expand research on alternative concepts, materials, and basic theory and experiments, it is also essential that evolution of the tokamak continue. Ensuring that it does will require that tokamak research remain a substantial part of the core program, and that this work remain connected to — and contribute to — the worldwide effort *in tokamak research. The two medium-scale tokamaks in the U.S. core program, DIII-D and Alcator C-MOD, have important roles to play in this connection. As indicated in Chapter 2, they are at productive stages in their operating lives, and their designs suit them for exploring a number of the important issues in advanced tokamak physics. These devices cannot do everything in advanced tokamak physics that TPX could do — most importantly they cannot explore the physics of steady-state operation. But they can both be operated for considerably less money than it would take to construct TPX. Even under an overall fusion-energy R&D budget constrained at $320 million per year, we believe that continuing to operate both DIII-D and Alcator C-MOD is highly desirable. The proposed DIII-D upgrade, which would increase this machine's capabilities in some of the forefront areas in tokamak physics research, is also desirable, although our constrained-budget case would require that the upgrade be delayed 3 years (to coincide with the shutdown date we recommend below for TFTR). Smaller tokamaks than DIII-D and Alcator C-MOD are also needed to help address a variety of critical fusion issues. Examples of critical issues that can be investigated fruitfully at small scale include disruptions, current drive, and transport.*

- **continue to operate TFTR for 3 years beyond its currently scheduled shutdown at the end of FY1995, at a somewhat reduced funding level of about $50 million per year;**

This recommendation is aimed at reaping additional scientific benefits from continued D-T operation in TFTR and supports the key priority of ignition and sustained burn as the major next-step scientific issue. TFTR is unique in the world program in its capability for extended D-T operation. The past year of TFTR experimentation has advanced the frontier of fusion research with regard to alpha-particle physics, the further understanding of which is a key motivation behind the international ignition and sustained burn experiment. Results obtained in TFTR over the next 3 years will yield new information on alpha-particle effects and will influence the physics plan for the ignition experiment.

The results from TFTR already have partly confirmed several theoretical predictions, but an unexpected positive result has also materialized: an improvement in plasma confinement apparently resulting from the presence of alpha particles. The TFTR team has developed an excellent plan that will creatively and significantly extend the device's capabilities. Through enhancement of the available plasma-control techniques, TFTR's fusion power output can be increased from 10 megawatts to 20 megawatts; this increase will double the population of alpha particles, enhance the alpha particle heating of the plasma, and approach some predicted limits for alpha-particle excitation of instabilities. In addition, novel techniques to control alpha-particle heating of the plasma and alpha-particle transport will be tested for the first time.

These experiments will not require any major upgrades of the TFTR systems. Accumulated operation by the end of the 3-year extension will not exceed that for which the device was designed, and the experiment is already fitted with outstanding and specialized diagnostic instrumentation to measure alpha-particle dynamics. Given the uniqueness of the TFTR capability, the importance of alpha-particle physics, and the promising plans for the next 3 years, we believe that continuation is highly desirable. In the constrained $50 million annual budget for the operation of this machine at Princeton (the FY1995 budget is about $62 million), *TFTR can probably operate effectively, but the full 3-year extension will be required to exploit its capabilities adequately.*

- continue U.S. participation in the Engineering Design Activities phase of ITER at the current level ($70 million per year), to which this country is committed through FY1998 under existing international agreements.

Under this proposed, budget-constrained program, the United States would also immediately open negotiations with its ITER partners to modify the post-FY1998 phase of international cooperation, seeking to

- gain agreement for downsizing ITER construction and operation from a $10-13 billion ignition-and-long-burn physics and reactor-technology development project to a not more than $4 billion ignition-and-moderate-burn physics project, on a construction timetable delayed 3 years from the current plan;

- promote the possibility of significant international participation in the complementary next-generation fusion experiments hitherto planned as domestic projects (such as TPX in the United States); and

- add to the collaborative agenda a materials/blanket test facility, as part of the international, low-activation-materials and blanket-development program. The United States should be prepared to commit up to a total of $200 million as its share of a project that achieves international consensus and begins construction in FY2000.

The expectation of a successful outcome from this negotiation would depend on the United States bringing to the table a firm commitment, endorsed by the President, of a $1.2 billion contribution to the next phase of the cooperation (cumulative over about 10 years). The negotiation would include the possibility of expanding the number of partners (to include, e.g., China, India, South Korea). It is possible that the outcome of the negotiation would be that the full-scale ITER was constructed despite the reduction of the U.S. contribution from what had been anticipated. This outcome would have the benefit of gaining, for the world, the additional science results and the technology-testing benefits associated with ITER as currently envisioned; but it would have the liability of sharply reducing the chance that money will be found within the international effort to fund the international materials test facility and to help pay for TPX (or another machine with a similar mission).

The $1.2 billion figure represents, nominally, $1 billion for a one-quarter U.S. share of a $4-billion ignition-and-moderate-burn experiment plus $200 million for the U.S. share of an international materials/blanket test facility. In the event that a suitable ignition experiment could be agreed and designed for less than $4 billion, or in the event that the international pool of funds for the collaboration was larger than 4 x $1.2 billion = $4.8 billion despite the limitation of the U.S. contribution to $1.2 billion (as could happen, for example, if additional partners were recruited), then it might become possible to fund TPX or a similar device from this international pool along with the ignition experiment and materials/blanket test facility.

Achievement of ignition and sustained burn of a plasma confined by a magnetic field has been for many years the preeminent goal of the world fusion community. Scientifically, it is the largest single physics unknown for a tokamak power system. In an ignited plasma, external heating can be turned off and the plasma will undergo fusion burn continuously. Sustaining the burn is also critically important in order to understand issues that might influence either the length of the burn or the optimal means of controlling the power- production level during sustained burn. Once ignited, the plasma will follow as yet unexplored paths into new high-temperature plasma regimes. The alpha particles will influence the plasma behavior in ways that are difficult to predict, especially since important aspects of plasma behavior depend upon plasma waves and turbulence, the description of which is at the forefront of theoretical physics research.

The Panel views ignition and sustained burn as the most important endeavor to pursue in a single major new experiment and believes that it should be undertaken internationally. In the ITER project as currently envisioned, however, combining an ignition experiment with a burn as long as 1,000 seconds — and with a technology testing phase intended to demonstrate most of the technological features of a reactor — has led to a project with a price-tag that may be higher than even an international consortium is willing to pay. Under any assumptions about future U.S. fusion R&D budgets consistent with the current budget-cutting climate, the United States will not be able to afford participation in the construction phase of the currently envisioned ITER as a full partner. It is also possible that the project will ultimately prove too costly for some of the other partners, even if the United States professes its willingness to continue with it at the initially intended scale. We think the prospects for actually carrying out an international ignition experiment in a timely way would be improved by downsizing the ITER project now, retaining for it the crucial mission of ignition and moderately sustained burn and separating out, to be accomplished in devices to be built elsewhere (and, in some cases, later), the long-burn and technology-testing missions. This is therefore the aim with which we think the United States should enter negotiations with its partners about the future of ITER.

The figure of $4 billion for a downsized ITER ignition-and-moderate-burn experiment is not based on a specific design; undertaking such a design in even a preliminary way would have been far beyond the scope of this Panel. The cost figure emerges, rather, from very rough considerations of the savings likely to be associated with eliminating the technology-testing components of ITER and with scaling down the physics experiment in both power and duration of pulse. Aiming for a pulse length of 50 to 100 seconds rather than a 1000-second burn would probably permit operating with copper coils (probably nitrogen cooled) instead of with superconducting coils. This would reduce the need for shielding of the coils and would permit a smaller device. That a machine considerably smaller than ITER would still be able to achieve ignition is suggested by the PPPL designs for the Compact Ignition Tokamak (CIT) and Burning Plasma Experiment (BPX). The BPX had an estimated total project cost (in 1992) of $1.7 billion (in as-spent dollars). Although the objective of BPX was operation at Q of 5 or more and a pulse length of about 5 seconds , the margin between the BPX cost figure and the $4 billion mentioned above allows some room for a design to be developed that would inspire high confidence in its capacity to achieve the dual goals of ignition and moderately sustained burn. The $4 billion figure has the further attraction of permitting the United States to play something like a 25%-partnership role in the project within an overall contribution to international collaborative efforts of about $1 billion. We believe this figure is the largest amount compatible *with the constrained U.S. fusion R&D budgets now in prospect. The actual cost of a downsized ITER and the details of a design for it, of course, would have to emerge from negotiation among the partners and an associated design process.*

As the last two of the four bulleted recommendations just above indicate, what we are proposing here really amounts to a restructuring of the international fusion collaboration as a whole, with the aim of optimizing the capacity of collaborative and domestic programs together to cover the combination of ignition-and-moderate-burn demonstration, advanced tokamak physics demonstrations, alternative-concepts research, and materials development that will be needed to progress toward an attractive reactor. We believe that if the United States brings to the negotiating table a firm commitment of a contribution of $1.2 billion to the next decade of fusion collaboration - and here the "firmness", underpinned by a Presidential endorsement, is likely to be essential — there is a good chance of an outcome in which the downsized ITER goes forward and it is also possible to build internationally a materials/blanket test facility and to fund, on an international basis, the construction of TPX or another steady-state advanced-physics tokamak. Such an outcome would become still more likely if the number of partners could be increased, which might be made possible by the growing interest in fusion in other major countries not now participating in ITER.

As noted above, it is also possible that the other ITER participants will decide — despite the efforts of the United States in the new negotiation and despite the reduction of the U.S. contribution from what had been expected — to proceed with construction of the full ITER as currently envisioned. As also noted, if this effort succeeded it would have the obvious merit of gaining the scientific and technological benefits toward which the current ITER has been designed; but proceeding in this direction would have the liability of absorbing so high a proportion of the total funds available in the world for fusion R&D that the chances of building either an international materials/blanket test facility or a steady-state, advanced-physics tokamak would be considerably reduced. That is why we think the U.S. preference should be for a downsized ITER. We did not think it would be fruitful or even feasible, however, for our Panel to try to anticipate all the twists and turns such a negotiation might take, and to make recommendations about what the U.S. position should be in each eventuality. Clearly, some flexibility as well as ingenuity will be required. We hope, though, that the sense of priorities we have tried to convey here will be useful to the U.S. side as the negotiation evolves.

We do consider it essential, in the $320 million per year scenario, that the United States continue to honor its formal commitment to participate fully in the ITER engineering- design phase that ends in FY1998; we also believe that $70 million per year meets that commitment. If the United States opens the discussion of downsizing the next phase of ITER by announcing that this country is withdrawing from its official commitment to complete the current phase, it loses credibility as a negotiating partner and potentially reliable participant in whatever next phase might be agreed. Although a $70 million per year "entry fee" to these discussions may seem high, the money being spent in ITER engineering design is producing insights about fusion technology that will be valuable whether or not ITER goes forward in the form currently planned. We recommend that, as part of the negotiation about the mission and scale of ITER, the United States should take the position that some of engineering design funds be spent exploring how to redesign ITER for a downsized role.

The rationale for adding to the collaborative agenda a materials/blanket test facility, as part of the international low-activation-materials and blanket-development program, is that (a) achieving fusion's highest potential requires a strong materials and blanket component development program and (b) progress will be made more rapidly if this program is international in scope, is well-coordinated, and has as a primary feature the construction of a large-scale facility as an international partnership in the spirit of the ITER agreement. The world fusion program has long studied the design of a small-volume, high-flux, accelerator-based fusion neutron source facility (the materials test facility or MTF) for small-sample irradiation testing. It has also described a larger volume neutron source (VNS) facility for the development and testing of blankets and other major fusion-plant core components. The Panel has not had the time or the charge to investigate the design viability of such specific facilities. We are convinced, however, that the development of low-activation materials and of critical fusion-power core components such as blankets is crucial to fusion's long term attractiveness. Thus, the Panel's recommendation is that the U.S. participate as a key international partner in a materials/blanket test facility and that it be prepared to commit up to a total of $200 million as its share of such a project that achieves international consensus and begins construction in FY2000.

During the negotiation of the next phase of the ITER cooperation, construction of TPX (currently scheduled to begin in FY1996) would be delayed for 3 years. Thereafter, TPX construction would proceed *if*

(a) the outcome of the negotiation was such as to permit funding the (probably downsized) ITER ignition experiment, the materials test facility, *and* the TPX with a cumulative contribution of $1.2 billion from the United States toward the total construction costs of these three facilities, with the remainder to come from our international partners,

or

(b) the outcome of the negotiation was such that the United States did not become a participant in an international ignition experiment, but an ignition experiment went forward somewhere under other auspices.

If neither of these outcomes occurred, construction of TPX would not proceed unless and until a review of the new situation — with its lack of a commitment to an ignition experiment anywhere — concluded that proceeding with TPX was the most sensible next step for the United States in that situation.

Under some of the possible outcomes from the negotiation of the next phase of international collaboration, TPX would not be built. This would be extremely unfortunate. We consider TPX to be a well conceived and innovative advanced tokamak experiment, without which the United States will lack a large tokamak of its own after TFTR is shut down. We believe, nonetheless, that the highest priority should be given to preserving both (a) U.S. participation in a robust international collaboration that includes, above all, an ignition experiment and a materials test facility, and (b) a strong domestic core program of theory and smaller experiments. If negotiation of the next phase of international

collaboration under a total U.S. contribution of $1.2 billion does not produce an outcome that achieves these ends and TPX as well, then the loss of TPX will have to be considered a particularly dismaying consequence of constraining the overall U.S. fusion R&D program to $320 million per year.

The steady-state advanced tokamak concept is embodied in the TPX experiment. The investigation of steady-state plasmas is a crucial step in both the physics and technology development for fusion. The development of the advanced tokamak concept is also likely to be essential if the tokamak is to evolve into an attractive reactor. The integration of steady state and advanced tokamak goals into one experiment is sensible, because a complete test of advanced tokamak ideas requires their demonstration in steady state. We believe that TPX represents an exciting and important fusion physics experiment, well suited to be the lead domestic experiment in the United States.

The cost of TPX would be roughly equal to that of a U.S. share in an international experiment focused on ignition physics. An aggressive fusion program, which we would endorse, would support both experiments. They are complementary to each other, and both are essential in pursuit of attractive fusion reactors for the middle of the next century. If, however, the budgets for U.S. fusion R&D become constrained to a degree that allows only for the core activities plus one large experiment, it will be necessary to decide which of the two experiments — the international ignition experiment or the domestic steady-state experiment — deserves higher priority. Although, as discussed above, it may be possible to finesse having to face this choice under some combinations of domestic budget stringency and reshaped international collaboration, if forced to face it we would assign the international ignition experiment higher priority than the domestic, steady-state, advanced tokamak.

We reach this conclusion regarding relative priorities while fully aware of three counterarguments:

- *It would be useful to explore ways to make tokamaks more compact and economical before pursuing ignition. In this way, the cost of the ignition device might be reduced, and it would be more likely to resemble an attractive reactor.*
- *Without TPX, the United States would have no cutting-edge tokamak of its own after TFTR shuts down. This would be a significant liability for the domestic program.*
- *TPX is ready to enter the construction phase now. An international ignition device, by contrast, is unlikely to go on line until at least seven years after the time TPX could begin operation.*

All of these are reasonable arguments, but we find the arguments in favor of pursuing ignition and in favor of U.S. participation in international collaborative approaches to be more compelling. It is also germane that, although none of the other existing or proposed experiments (and no combination of these) can fully perform the TPX mission, the other experiments can perform parts of it. The ignition mission, by contrast, cannot be addressed even approximately by any existing machine.

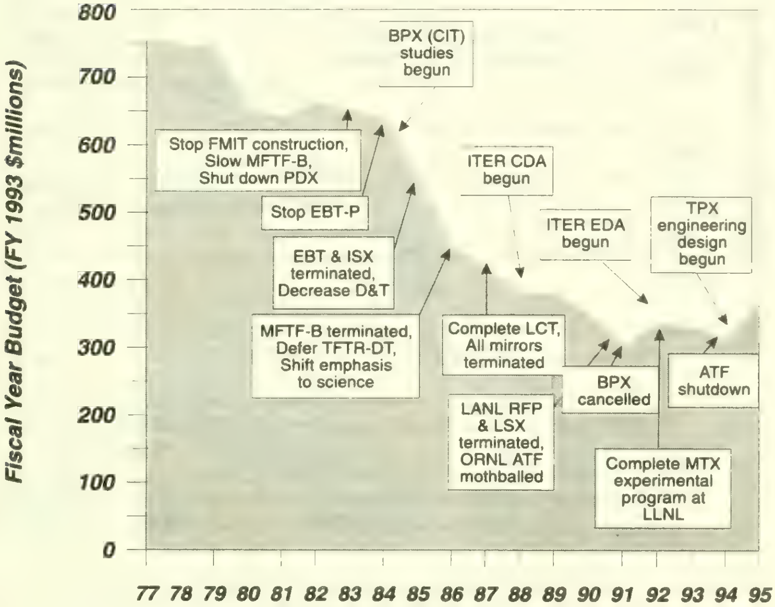
Although TPX would only be constructed, in our $320 million per year scenario, under the subset of possible outcomes of international negotiations indicated in the boldfaced text just above, we recommend that a reduced TPX R&D effort by the core national TPX team continue during the international negotiation phase. The purposes of the continued effort are to move forward in the TPX design, to continue the intellectual leadership provided by the TPX project in advanced tokamak research, and to maintain a state of readiness for construction in the event that the outcome of the negotiation makes it feasible to proceed. Industrial involvement in the effort should continue, at a level appropriate to the reduced design effort.

In addition to developing the strategy just described for a fusion R&D program funded at about $320 million per year, we also have attempted to envision a program that could preserve key priorities at a still lower budget level of about $200 million per year. We find that this cannot be done. Reducing the U.S. fusion R&D program to such a level would leave room for nothing beyond the core program of theory and medium-scale experiments described above — no contribution to an international ignition experiment or materials test facility, no TPX, little exploitation of the remaining scientific potential of TFTR, and little sense of progress toward a fusion energy goal. With complete U.S. withdrawal, international fusion collaboration might well collapse — to the great detriment of the prospects for commercializing fusion energy as well as the prospects for future U.S. participation in major scientific and technological collaborations of other kinds. These severe consequences — deeply damaging to an important and fruitful field of scientific and technological development, to the prospects for achieving practical fusion energy, and to international collaboration in science and technology more generally — are too high a price to pay for the budgetary savings involved.

We urge, therefore, that the Administration and the Congress commit themselves firmly to a U.S. fusion R&D program that is stable at not less than $320 million per year.

* * * * *

U.S. MAGNETIC FUSION BUDGET AND PROJECT HISTORY
FISCAL YEARS 1977-95
(FY 1993 dollars in millions)

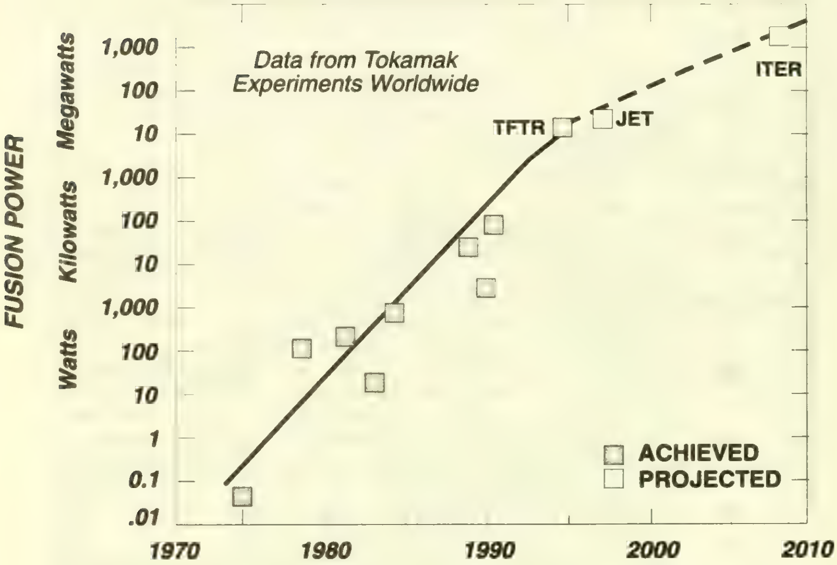


KEY
BPX/CIT = Burning Plasma Experiment/Compact Ignition Tokamak
DT = Deuterium-Tritium
EBT = Elmo Bumpy Torus
EBT-P = Elmo Bumpy Torus-P
FMIT = Fusion Materials Irradiation Test Facility
ISX = Impurity Studies Experiment (a tokamak)
ITER CDA = International Thermonuclear Experimental Reactor Conceptual Design Activities
ITER EDA = International Thermonuclear Experimental Reactor Engineering Design Activities
LANL RFP = Los Alamos National Laboratory Reverse Field Pinch
LCT = Large Coil Test Facility (superconducting magnets)
LLNL = Lawrence Livermore National Laboratory
LSX = Large S Experiment (a field-reversed compact toroid device)
MFTF-B = Mirror Fusion Test Facility-B
MTX = Microwave Tokamak Experiment
ORNL ATF = Oak Ridge National Laboratory Advanced Toroidal Facility (a stellarator)
PDX = Princeton Divertor Experiment
TFTR = Tokamak Fusion Test Reactor
TPX = Tokamak Physics Experiment

SOURCE U S Department of Energy 1994
Modified from Office of Technology Assessment Report of 2/95

Figure 1

PROGRESS IN MAGNETIC FUSION POWER



TFTR	Tokamak Fusion Test Reactor
JET	Joint European Torus
ITER	International Thermonuclear Experimental Reactor

Updated from Fusion Policy Advisory Committee Report of 9/90.

Figure 2

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Appendix A

PCAST REVIEW OF THE DOE MAGNETIC FUSION PROGRAM: CHARGE TO THE PANEL

The Department of Energy's magnetic fusion energy program has the stated goals of (i) developing a demonstration fusion power plant by approximately 2025 and (ii) facilitating the operation of a commercial fusion power plant by 2040. The research and development program that has been formulated by the DOE to meet these goals, funded in FY1995 at $368 million per year, would require very substantial increases in budget over the next decade; this will be problematic if Federal expenditures for energy research and development of all kinds remain constant or decline in this period, as is widely expected. The conference report for the Fiscal Year 1995 Energy and Water Appropriations Act points out this potential difficulty and calls for a review of the DOE's magnetic fusion energy program by the President's Committee of Advisors on Science and Technology (PCAST).

In response to this request, the Office of Science and Technology Policy has appointed a fusion review Panel, under the auspices of PCAST, consisting of four members of PCAST and five other members chosen for their diverse perspectives and expertise. The task of the Panel is to clarify the technical and policy tradeoffs and budgetary requirements associated with -- and recommend preferred alternatives among -- various possible trajectories for the magnetic fusion energy program, including, (a) the trajectory currently programmed, (b) an alternative in which expenditures would increase in a similar manner but would be allocated differently, (c) an alternative in which expenditures would remain approximately constant, (d) an alternative in which expenditures would decrease moderately, and (e) an alternative in which expenditures would decrease sharply.

Because of the large role, in the budgets for the currently programmed trajectory, of two major construction projects -- the Tokamak Physics Experiment (TPX), scheduled to be completed at Princeton in 2001, and the Intenational Thermonuclear Experimental Reactor (ITER), scheduled to be completed in 2008 at a site not yet determined -- consideration of alternatives must include the possibilities of downsizing, deferral, or elimination of one or both of these experiments. Also important in the evaluation of alternative scenarios will be the scope they provide for vigorous efforts in theoretical and experimental plasma physics independent of the large machines, for a suitable program to develop the special materials that fusion reactors will require, for efforts to identify and investigate reactor concepts other than the tokamak approach being pursued in TPX and ITER, and for continuation of the high degree of international collaboration that has been a hallmark of the global fusion effort for nearly 40 years.

The PCAST Panel is to report to the Office of Science and Technology Policy at the end of June 1995, in time to influence Congressional action on the Fiscal Year 1996 budget and to be used in the Administration's Fiscal Year 1997 budget decisions. This schedule does not permit including, in the Panel's mandate, any substantial review of the related program in inertial confinement fusion, which has been funded mainly under the DOE's defense programs for its applications to the study of nuclear-weapons physics. The Panel's review of programs exploring alternatives to the tokamak will include, however, a review of the modest effort funded on the energy side of DOE's budget to explore applications of inertial fusion technology as an energy source.

The Panel will draw upon, but will not be limited by, the findings of reviews of the DOE's magnetic fusion energy program conducted over the past few years by the Department's Fusion Power Advisory Committee (FPAC) and Fusion Energy Advisory Committee (FEAC), by the National Research Council, and by the Congressional Office of Technology Assessment, and will take cognizance of the parallel review of the full range of the DOE's strategic energy research and development programs being conducted by the "Yergin Committee" of the Secretary of Energy Advisory Board.

Appendix B

List of Principal Briefings

March 29, 1995: Washington, D.C.

John H. Gibbons, Assistant to the President for Science and Technology
Martha A. Krebs, Director, Office of Energy Research, DOE
Anne Davies, Associate Director for Fusion Energy, DOE
Robert Aymar, Director, ITER Project
Robert J. Goldston, Chief Scientist, Tokamak Physics Experiment, Princeton, N.J.

April 13-14: San Diego, California

Masaji Yoshikawa, Executive Director, Japan Atomic Energy Research Institute
Charles Maissoner, Director, Fusion Program, European Commission
Evgenii Velikhov, President, Kurchatov Institute, Russian Federation
Robert Aymar, Director, ITER Project
Charles C. Baker, ITER U.S. Home Team Leader, San Diego, California
Ben Carreas, Oak Ridge National Laboratory, Tennessee
Thomas C. Simonen, Director, D-III Program, General Atomics, San Diego, California
David E. Baldwin, Associate Director, Lawrence Livermore National Laboratory

April 24: Washington, D.C.

T.J. Glauthier, Associate Director, Office of Management and Budget
Stephen O. Dean, President, Fusion Power Associates

April 25: Princeton, N.J.

Ronald C. Davidson, Director, Princeton Plasma Physics Laboratory
Dale M. Meade, Deputy Director, Princeton Plasma Physics Laboratory
Gerald A. Navratil, Professor of Applied Physics, Columbia University

May 17-18, 1995: Washington, D.C.

John P. Boright, Deputy Associate Director, OSTP
Michael Campbell, Associate Director, Lawrence Livermore National Laboratory
Bruno Coppi, Massachusetts Institute of Technology (MIT)
Miklos Porkolab, Director, Plasma Fusion Center, MIT
Stephen L. Rosen, Director, Industry Relations, Houston Lighting & Power Co., Texas
Marshall Rosenbluth, Chief Scientist, ITER Project

June 12-14, 1995: San Francisco, California - Executive Session

Appendix C

Communications Received by the Panel

The PCAST Fusion Panel wishes to acknowledge with appreciation the letters and memorandums received from the following scientists and engineers who offered views and relevant information on the future of the fusion energy research and development program:

Aamodt, Richard E., Lodestar Research Corporation, Boulder, Colorado

Baldwin, David E., Senior Vice President for Fusion, General Atomics, San Diego, California

Berk, Herbert L., Professor of Physics, University of Texas, Austin, Texas

Bradbury, James, Los Alamos National Laboratory, New Mexico

Campbell, Michael, Associate Director, Laser Programs, Lawrence Livermore National Laboratory, California

Cary, John R., Professor and Chair, Astrophysical, Planetary, and Atmospheric Sciences, University of Colorado, Boulder, Colorado

Davidson, Ronald C., Director, Princeton Plasma Physics Laboratory, New Jersey

Drake, R. Paul, Plasma Physics Research Institute, Lawrence Livermore National Laboratory, California

Goldston, Robert J., TPX Chief Scientist, Princeton Plasma Physics Laboratory, New Jersey

Hammer, James H., Inertial Confinement Fusion Program, Lawrence Livermore National Laboratory, California

Hazeltine, R.D., Director, Institute for Fusion Studies, University of Texas, Austin, Texas

Hooper, E. Bickford, Magnetic Fusion Energy Program, Lawrence Livermore National Laboratory, California

Iotti, Robert C., International Thermonuclear Experimental Reactor (ITER) Project Office, San Diego, California

Landis, John W., Senior Vice President (Retired), Stone and Webster Engineering Corporation, Boston, Massachusetts

Longhurst, Glen R., Idaho National Engineering Laboratory, Idaho Falls, Idaho

Marton, Warren A., Office of Fusion Energy, DOE

Meade, Dale M., Deputy Director, Princeton Plasma Physics Laboratory, New Jersey

Navratil, Gerald A., Professor of Applied Physics, Columbia University, New York

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Appendix D

GLOSSARY OF FUSION TERMS AND ACRONYMS

Alcator-C	MIT, dense plasmas, high magnetic field; C-MOD= upgrade; exploring divertor solutions.
ASDEX	Axisymmetric Divertor Experiment, Garching, Germany; enhanced tokamak confinement and current drive.
ATF	Advanced Toroidal Facility, Oak Ridge National Laboratory (ORNL) stellerator, shut down 1994.
Beta	The ratio of the outward pressure exerted by the plasma to the inward pressure that the magnetic confinement field is capable of exerting.
Blanket	The structure surrounding the plasma in a fusion reactor, within which the fusion-produced neutrons are slowed down, heat is transferred to a primary coolant, and tritium is bred from lithium.
Break-even	The point at which the fusion power generated in a plasma equals the amount of heating power that must be added to the plasma to sustain its temperature (corresponds to Q= 1).
Burning plasma	A plasma in which the fusion reactions supply a significant fraction of the energy needed to sustain the plasma.
BPX	Burning Plasma Experiment (also called CIT), cancelled 1992.
CDX	Current Drive Experiment, Princeton.
CIT	Compact Ignition Tokamak, same as BPX.
Confinement	Restraint of plasma within a designated volume. In magnetic confinement, this restraint is accomplished with magnetic fields.
CPRF	Confinement Physics Research Facility, reversed field pinch, Los Alamos National Laboratory (LANL), same as ZT-H, terminated 1991, during construction.
D-D reaction	A fusion reaction in which one nucleus of deuterium fuses with another.
DEMO	Demonstration Fusion Power Reactor, target 2025 in National Energy Strategy.
Deuterium	A naturally occuring heavy isotope of hydrogen containing one proton and one neutron in its nucleus.

Divertor	A component of a toroidal fusion device used to shape the magnetic field near the plasma edge so that particles near the edge are captured before they can strike the walls and generate secondary particles that would contaminate and cool the plasma.
DIII-D	Doublet III-D, General Atomics, Beta=10%.
D-T reaction	A fusion reaction in which a nucleus of deuterium fuses with a nucleus of tritium. The D-T reaction is the most reactive fusion reaction.
EBT	Elmo Bumpy Torus, cancelled 1985.
ETF	Engineering Test Facility (inertial fusion), pre-DEMO.
ETR	Engineering Test Reactor (US generic term for ITER-scale device).
Field-reversed configuration	A magnetic confinement concept with no toroidal field, in which the plasma is essentially cylindrical in shape.
Fission	The process by which a neutron strikes a nucleus and splits it into fragments; during the process of nuclear fission, neutrons are released at high speed, and heat and radiation are released.
FMIT	Fusion Materials Irradiation Test Facility, halted 1983.
FT-U	Frascati Tokamak Upgrade, Italy, high density, high current $150 million class.
Fusion	The process by which the nuclei of light elements combine, or fuse, to form heavier nuclei, releasing energy.
HLT	High-performance Long-pulse Tokamak, European pre-conceptual design, copper coils.
Ignition	The point at which a fusion reaction becomes self-sustaining. At ignition, fusion self-heating is sufficient to compensate for all energy losses; external sources of heating power are no longer necessary to sustain the reaction.
ILSE	Induction Linac Systems Experiment, heavy-ion driver for inertial fusion, $50 million.
Inertial confinement	An approach to fusion in which intense beams of light or particles are used to compress and heat tiny particles of fusion fuel so rapidly that fusion reactions occur before the pellet has a chance to expand.
ISX	Impurity Studies Experiment, tokamak, ORNL, terminated 1985.
ITER	International Thermonuclear Experimental Reactor, 2005.
JET	Joint European Torus, Culham, UK, biggest tokamak, Q= 0.7.

JT-60	Japan, tokamak; U= upgrade (approaches JET); SU= superupgrade.
Lawson parameter	The product of plasma density and confinement time that, along with temperature, determines the ratio between power produced by the plasma and power input to the plasma.
LHD	Large Helical Device, Japan, stellerator, $1 billion.
LMF	Laboratory Microfusion Facility, 400-MJ class inertial fusion energy facility, possibly combined with ETF.
Low-activation materials	Materials that, under neutron irradiation, do not generate intensely radioactive, long-lived radioactive isotopes and produce less afterheat following a reactor shutdown than more conventional materials.
LSX	Large S Experiment (field-reversed compact toroid).
Magnetic confinement	Any means of containing and isolating a hot plasma from its surroundings by using magnetic fields.
Magnetic fusion energy	Energy released by a thermonuclear reaction in the fuel of a magnetically confined plasma.
Magnetic mirror	A generally axial magnetic field that has regions of increased intensity at each end where the magnetic fields converge.
MFTF-B	Mirror Fusion Test Facility, B, Lawrence Livermore National Laboratory (LLNL), shut down 1986.
MST	Madison Symmetric Torus, reversed-field pinch, U of Wisconsin.
MTX	Microwave Tokamak Experiment, LLNL.
NIF	National Ignition Facility, LLNL, laser fusion (planned), yield 1-20 MJ.
$nT\tau$	The product of the key figures of merit relating to plasma quality: density (n) times temperature (T) times energy confinement time (τ).
PBX-M	Princeton Beta Experiment (M = modification), approached second stability.
PDX	Princeton Divertor Experiment, shut down 1983.
Plasma	An electrically neutral gas of charged particles.
PLT	Princeton Large Torus, predecessor of TFTR.
Q	The ratio of fusion power (P_f) generated in the plasma to the heating power that must be added to the plasma to sustain its temperature.

Reversed field pinch	A closed magnetic confinement concept having toroidal and poloidal magnetic fields that are approximately equal in strength, and in which the direction of the toroidal field at the outside of the plasma is opposite from the direction at the plasma center.
RFX	Reversed-Field (pinch) Experiment, Italy, operation 1991.
SBX	Steady Burn Experiment, potential upgrade from SS/AT.
Spheromak	A compact toroidal magnetic confinement concept in which a large fraction of the confining magnetic fields is generated by currents within the plasma.
SS/AT	Steady State Advanced Tokamak, Princeton, same as TPX.
Stellarator	A toroidal magnetic confinement device in which the confining magnetic fields are generated entirely by external magnets.
T-10	Russia, tokamak, 2 MW electron-cyclotron heating power.
T-15	Russia, Kurchatov, large superconducting tokamak.
TEXT	Texas Experimental Tokamak, U of Texas, Austin.
TEXTOR	Torus Experiment for Technology-Oriented Research, Julich, Germany.
TFTR	Tokamak Fusion Test Reactor, Princeton.
Tokamak	A magnetic confinement concept whose principal confining magnetic field, generated by external magnets, is in the toroidal direction but that also contains a poloidal magnetic field that is generated by electric currents running within the plasma.
Tore Supra	Superconducting toroidal field coil tokamak, Cadarache, France, 1988.
Toroidal	In the shape of a torus (i.e., doughnut-shaped).
TPX	Tokamak Physics Experiment, Princeton, same as SS/AT, low-budget replacement for BPX.
Tritium	A radioisotope of hydrogen that has one proton and two neutrons in its nucleus. In combination with deuterium, tritium is the most reactive fusion fuel.
TSTA	Tritium Systems Test Assembly, LANL.
VNS	Volume Neutron Source, pre-conceptual alternative to blanket-testing.
Wendelstein-7AS/X	Wendelstein stellarators, Garching, Germany.

DOE/ER-0684

Strategic Plan for the Restructured U.S. Fusion Energy Sciences Program

August 1996

U. S. Department of Energy
Office of Energy Research
Office of Fusion Energy Sciences
Germantown, MD 20874-1290

Executive Summary

This plan reflects a transition to a restructured fusion program, with a change in focus from an energy technology development program to a fusion energy sciences program.

Since the energy crisis of the early 1970s, the U.S. fusion program has presented itself as a goal-oriented fusion energy development program, with milestones that required rapidly increasing budgets. The Energy Policy Act of 1992 also called for a goal-oriented development program consistent with the Department's planning. Actual funding levels, however, have forced a premature narrowing of the program to the tokamak approach. By 1995, with no clear, immediate need driving the schedule for developing fusion energy and with enormous pressure to reduce discretionary spending, Congress cut fusion program funding for FY 1996 by one-third and called for a major restructuring of the program.

Based on the recommendations of the Fusion Energy Advisory Committee (FEAC), the Department has decided to pursue a program that concentrates on world-class fusion science, with innovation in all areas of fusion research, on world-class plasma science, and on maintaining an involvement in fusion energy science through international collaboration.

At the same time, the Japanese and Europeans, with energy situations different from ours, are continuing with their goal-oriented fusion programs. Collaboration with them provides a highly leveraged means of continued involvement in fusion energy science and technology, especially through participation in the engineering and design activities of the International Thermonuclear Experimental Reactor program, ITER.

This restructured fusion energy sciences program, with its focus on fundamental fusion science and technology, may well provide insights that lead to more attractive fusion power plants, and will make use of the scientific infrastructure that will allow the United States to launch a fusion energy development program at some future date.

The Conference Report accompanying the FY 1996 Energy and Water Development Appropriations Act provided guidance on the need for the fusion program to restructure its strategy, content, and near-to-medium term objectives. It instructed the Department of Energy (DOE), with the participation of the fusion community and FEAC, to prepare a strategic plan to implement such a restructured program at a constant level of effort for the next several years. This Strategic Plan responds to the congressional guidance. In this Plan, DOE endorses the new mission and policy goals contained in the recent FEAC Report and outlines the specific steps that DOE is taking to implement the substantial restructuring of the Fusion Energy Sciences Program that was recommended by FEAC in response to congressional guidance.[1]

The new *MISSION* of the Fusion Energy Sciences Program is:

> ***Advance plasma science, fusion science, and fusion technology — the knowledge base needed for an economically and environmentally attractive fusion energy source.***

The *POLICY GOALS* that support the new mission are:

- ***Advance plasma science in pursuit of national science and technology goals;***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program; and***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

The previous strategy was characterized as a schedule-driven development program to prove fusion to be a technically and economically credible energy source with the goal of an operating demonstration power plant by about 2025. However, in a climate of severe budgetary constraints, the previous strategy became highly unrealistic in terms of the funding increases that would have been required to attain such a goal. In the face of budgetary constraints, the fusion program concentrated almost all of its available resources on the conventional tokamak concept, virtually eliminating support for alternative concepts and plasma science.

The new strategy emphasizes advancing the scientific knowledge base, including basic plasma science, needed for an economically and environmentally attractive fusion energy source, with the United States playing an important supporting role as a partner in the international pursuit of fusion energy development. To be a credible partner in this long-term quest, the United States needs a vigorous domestic program in fusion science and technology. At a constant level of funding, the restructured U.S. program will focus on fusion's underlying scientific foundations and will enable the United States to exert leadership in selected areas of expertise in the international effort to develop fusion energy.

Highlights of the restructured Fusion Energy Sciences Program are:

Advance Plasma Science - The fusion program will assume the responsibility to be the advocate for and to act as a steward for basic plasma science, which is the underlying core science of fusion energy and has significant impact on a wide range of national science and technology goals (e.g., national security, industrial processing, and astrophysics, as well as fusion energy).

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[1] ***A Restructured Fusion Energy Sciences Program*****, Report of the Fusion Energy Advisory Committee, January 27, 1996.**

Develop Fusion Science and Concept Innovation - The restructured program will pursue the underlying science and enabling technology of fusion, with increased emphasis on conventional tokamak fusion concept innovation, alternative concepts, theory and computation, materials and key enabling technologies, and inertial fusion energy. Although each of the three major experimental tokamak facilities has distinct capabilities and makes different contributions to fusion science, it will be necessary to terminate one of these facilities in the near-term in order to provide resources for the program initiatives. The DOE concurs with the FEAC recommendation that the Tokamak Fusion Test Reactor should be the first of the tokamaks to be retired, after a period of operation to extract the remaining scientific benefit.

Pursue Fusion Energy as an International Collaboration - Fusion energy offers the long-term potential to provide an environmentally attractive and economically viable energy option for a growing world population in the next century. Although the United States is unable to pursue an independent fusion energy development program, it is in the national interest to remain a credible partner in the international fusion program aimed at long-term energy development. The restructured program seeks to meet the U.S. commitment to the successful completion of the Engineering Design Activities (EDA) for ITER in July 1998, which will leave open the possibility of U.S. participation in ITER construction, as a modest cost but high-leveraged investment.

Resources - The FY 1996 budget of $244 million (a 32% reduction compared to FY 1995) required difficult choices among: meeting U.S. international commitments to the ITER EDA; conducting scientific programs at major world-class research facilities located within the United States (facilities that are in a period of unprecedented scientific productivity); and terminating valuable elements of the core U.S. scientific program. The Department's FY 1997 budget submission has restructured the fusion program as recommended by FEAC. This Strategic Plan assumes that the restructured program will be implemented at a constant level of effort of about $255 million per year. In FY 1997, funds for Program Direction and for computing are budgeted elsewhere in the Energy Research Energy Supply, Research, and Development program. The comparable FY 1996 funding level is $227.4 million. The comparable FEAC recommendation is $264 million.

Program Governance - The Office of Fusion Energy Sciences is being reorganized, and the number of people in the Office is being reduced to be consistent with its new science mission. A new decision-making process involving more fusion community input, more peer review, and the assistance of the FEAC Scientific Issues Subcommittee is being installed. The peer review process will be used as the primary mechanism for evaluating proposals, assessing progress and quality of work, and for initiating and terminating facilities, projects, research programs, and groups.

Strategic Plan for the Restructured U.S. Fusion Energy Sciences Program

Introduction

Since the energy crisis of the early 1970s, the U.S. fusion program has presented itself as a goal-oriented fusion energy development program, with milestones that required rapidly increasing budgets. Funding did increase from the mid-1970s until about 1980, when the urgency associated with new energy sources evaporated. Based on the Fusion Policy Advisory Committee review of 1990, the Department put forward specific, long-term energy goals for the program, goals that, again, required increasing funding. For years, in the face of flat or declining budgets, the program has maintained its long-term goals and its mission, planning for increased funding in the near future. Doing so has required an ever narrowing research program focused more and more on the tokamak concept. By 1994, the mismatch between the program's stated goals and the available funding had become clear, and Congress called for a review of the program by the President's Committee of Advisors on Science and Technology (PCAST).

In the summer of 1995, both the PCAST and the Congress made it clear that the mission and the goals of the fusion program should change. In particular, PCAST recommended a program without the energy specific milestones, such as the operation of a demonstration power plant or a commercial plant. With no clear, immediate need driving the schedule for developing fusion energy and with enormous pressure to reduce discretionary spending, Congress cut funding for the fusion program by one-third for FY 1996, and called for a major restructuring of the program.

With the involvement of the fusion community, the Fusion Energy Advisory Committee (FEAC) has recommended and the Department has adopted a new strategy for this program, a strategy that builds upon the PCAST report, but goes beyond it to focus on the science that underpins the fusion process. This new strategy, with a still substantial budget, will allow the program to explore alternative approaches to fusion, to pursue innovation in all areas of fusion research, and to maintain an involvement in fusion energy science. The program will now work toward identifying a more attractive end product and, perhaps, a more affordable development path, while conducting world-class fusion and plasma science.

At the same time, the Japanese and Europeans, with energy situations different from ours, are continuing with their goal-oriented fusion programs. Collaboration with our international partners provides a highly leveraged means of continued involvement in fusion energy science and technology. Although reduced in scale, the restructured U.S. program will continue to have much to offer our international partners and much to gain, especially through participation in the International Thermonuclear Experimental Reactor program, ITER, where the experimental exploration of the physics of burning plasmas, an essential element of fusion science, will occur.

This combination of fusion and plasma science, innovation, and the search for more attractive approaches, along with international collaboration on fusion energy science, will allow the United States to maintain the fusion infrastructure and many of the competencies that will provide the platform from which to launch a fusion energy development program at some future date. That time will come when the development path is judged to be affordable and the end product attractive enough to justify the cost.

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The Conference Report accompanying the FY 1996 Energy and Water Development Appropriations Act[1] provided guidance on the need for the fusion program to restructure its strategy, content, and near-to-medium term objectives and instructed the Department of Energy (DOE), with the participation of the fusion community and FEAC, to prepare a strategic plan to implement such a restructured program at a constant level of effort for the next several years.

In response to the congressional guidance, the Director of the Department's Office of Energy Research requested FEAC to provide recommendations on how to restructure the fusion program. The Department also requested that, in its deliberations on the restructuring of the fusion program, FEAC consider the broader issue of plasma science that underpins fusion energy and has numerous applications in science, technology, and the commercial sector. In addition, DOE requested FEAC's help in planning the implementation of the strategy for restructuring the fusion program, including institutional considerations and the role of international collaboration in the program.

On January 27, 1996, FEAC transmitted its report, "A Restructured Fusion Energy Sciences Program" to DOE.[2] The FEAC Report provided recommendations on how to restructure the fusion program in light of congressional guidance and budgetary realities. The FEAC recommended a new mission and new policy goals for a renamed Fusion Energy Sciences Program. The FEAC pointed out that the fusion energy program outlined in the Energy Policy Act of 1992 with the goal of a technology demonstration by 2010 that would verify the practicability of commercial electric power production, cannot be realized at the budget levels now foreseen. As a result, FEAC recommended that the mission of the U.S. program be restated in a world context to reflect the reality that the leadership of the world's fusion energy development effort now lies outside the United States and to emphasize the program's science and technology goals.

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[1] *H.R. Conference Report Number 293, 104th Congress, 1st Session, 62 (1995).*

[2] ***A Restructured Fusion Energy Sciences Program*, Report of the Fusion Energy Advisory Committee, January 27, 1996.**

The goals recommended by FEAC have the same major elements as the key priorities contained in the July 1995 Report[3] of PCAST, but emphasize more the science foundation of the program and take into account later congressional funding guidance to conduct the program within a reduced annual budget.

The FEAC recommended *MISSION* is:

> ***Advance plasma science, fusion science, and fusion technology -- the knowledge base needed for an economically and environmentally attractive fusion energy source.***

In pursuit of the new mission, FEAC recommended the following *POLICY GOALS*:

- ***Advance plasma science in pursuit of national science and technology goals;***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program; and***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

The Department endorses the FEAC recommended Mission and Policy Goals.

This Strategic Plan discusses these policy goals and outlines the specific steps that DOE is taking to implement the substantial restructuring of the Fusion Energy Sciences Program that was recommended by FEAC in response to congressional guidance.

Overall Change in Strategy

The previous strategy was characterized as a schedule-driven development program to prove fusion to be a technically and economically credible energy source, with the goal of an operating demonstration power plant by about 2025. However, in a climate of severe budgetary constraints, the previous strategy became highly unrealistic in terms of the funding increases that would have been required to attain such a goal. In an attempt to stay as close as possible to this goal-oriented schedule in the face of budgetary constraints, the fusion program concentrated almost all of its available resources on the tokamak concept, virtually eliminating support for alternative concepts and restricting funding for concept improvement and plasma science. Despite impressive scientific progress, the program has continued to receive insufficient resources to accomplish its mission.

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[3] ***The U.S. Program of Fusion Energy Research and Development, Report of the Fusion Review Panel,*** **President's Committee of Advisors on 'Science and Technology, July 1995.**

The new strategy emphasizes advancing the scientific knowledge base, including basic plasma science, needed for an economically and environmentally attractive fusion energy source, with the United States playing an important supporting role as a partner in the international pursuit of fusion energy development. To be a credible partner in this long-term quest, the United States needs a vigorous domestic program in fusion science and technology. At a constant level of funding, the restructured U.S. program will focus on fusion's underlying scientific foundations and will enable the United States to exert leadership in selected areas of expertise in the international effort to develop fusion energy.

Advance Plasma Science

The underlying core science of fusion energy is plasma science, which is the study of the ionized states of matter and is central to the development of fusion as an energy source. As documented in the recent National Research Council (NRC) Plasma Science Report, plasma science is a fundamental scientific discipline that has significant impact on a wide range of national science and technology goals (e.g., national security, industrial processing, and astrophysics, as well as fusion energy).[4] Citing the many important future contributions to our society that can be expected from a healthy plasma science program, the NRC Report expressed a central concern about the need to address effectively the current lack of support for fundamental plasma science. The NRC Report recommended that DOE's Office of Basic Energy Sciences, with the cooperation of the Office of Fusion Energy, "... provide increased support for basic experimental plasma science."

The containment of high temperature plasmas required for the production of fusion energy is the primary reason for the fusion program to support plasma science. According to the NRC Report, fusion energy is "the largest driver for the intellectual development of plasma science." However, under the prior strategy of schedule-driven development, when faced with budget constraints the fusion program narrowed support for plasma science, funding only that science directly applicable to fusion technology (i.e., high temperature magnetized plasma physics).

A key policy goal of the restructured fusion program is to advance plasma science in pursuit of national science and technology goals. To achieve this policy goal, the fusion program will assume the responsibility to advocate and act as a steward for basic plasma science. The fusion program plans to expand its support to include fundamental plasma science, regardless of potential future application, in partnership with other DOE offices and other agencies. The expected benefits to the fusion programs, both magnetic and inertial, from this new responsibility include aiding the development of fusion energy through advances in fundamental understanding of plasmas, and an improved interaction with related disciplines in the scientific community. Benefits should also accrue to other DOE energy programs, as this basic research may well lead to improved materials and processes with broad applications.

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[4] *Plasma Science From Fundamental Research to Technological Applications*, National Academy Press, Washington, D.C., 1995.

Develop Fusion Science and Concept Innovation

The restructured fusion program will pursue the underlying science and enabling technology of fusion, with increased emphasis on conventional tokamak concept innovation, alternative fusion concepts, theory and computation, materials and key enabling technologies, and inertial fusion energy. Tokamak research will continue to be a major element in the science program because tokamaks provide a superb vehicle for experiments in fusion science and because innovative conventional tokamak research offers the potential for significant improvements in the concept.

Conventional Tokamak Concept Innovation:

> The fusion program's major tokamak research facilities are DIII-D at General Atomics in San Diego, Alcator C-MOD at the Massachusetts Institute of Technology, and the Tokamak Fusion Test Reactor (TFTR) at the Princeton Plasma Physics Laboratory (PPPL). These are world-class research facilities with outstanding records of scientific accomplishment. Their research programs contribute greatly to the goal of tokamak concept improvement by establishing the scientific foundations for steady-state operation, low frequency of disruption, improved confinement, and higher power density. These programs also contribute stimulating ideas and results to the world fusion program. Each facility has distinct capabilities that enable it to make different contributions to fusion science. However, restructuring the fusion program within greatly reduced budget levels will make it necessary to terminate one of these facilities in the near-term to provide resources for new initiatives. The Department concurs with the FEAC recommendation that "TFTR should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from this facility."

Alternative Fusion Concepts:

> This refers to magnetic confinement approaches other than the conventional tokamak. Under the prior strategy, in response to budgetary pressure, in 1990 U.S. support for alternative concepts was drastically reduced in favor of schedule-driven development of the tokamak reactor concept. The restructured program will place renewed emphasis on broadening the scope of research to include studies of alternative concepts. The principal reason for investigating

alternative concepts is to advance fusion science in ways not possible with the tokamak concept alone. Understanding the scientific issues involved may also lead to improved concepts for energy applications.

Theory and Computation:

The United States is still a world leader in theory and computational modeling, which, in conjunction with experiment, provide the predictive capability needed for the conduct of scientific research. The restructured fusion program will increasingly build upon U.S. strengths in theory and modeling to achieve scientific and technological goals in a cost-effective way. At low cost, theory and modeling, which have made great strides in recent years, can have enormous impact on the program by influencing the direction of large, costly experimental facilities.

Materials and Key Enabling Technology:

Fusion science includes research on materials and key enabling technologies (e.g., radiation resistant, low-activation materials essential to achieve the safety and environmental potential of fusion; blanket system technologies that meet safety and performance requirements; and enabling technologies required to support advances in plasma physics). The science associated with fusion materials and technology research and development has a wide range of applications far beyond the fusion program (e.g., superconducting magnet technology, high temperature and radiation resistant materials, and cryogenic materials). The restructured U.S. fusion program will focus on those materials and technology issues that are critical to the safety and environmental goals of fusion and will strive for extensive use of international cooperation.

Inertial Fusion Energy (IFE):

In inertial confinement fusion, the fusion energy is released by imploding a small pellet of deuterium and tritium using energetic lasers or particle beams as drivers. Inertial confinement fusion is primarily supported by Defense Programs (DP) in DOE to provide the scientific base for national security applications. The Office of Fusion Energy Sciences (OFES) has been funding a relatively small research effort (about $8 million per year) on energy-specific efforts, primarily heavy ion beam development for inertial fusion energy. The FEAC Report did not assess the IFE effort in detail, but acknowledged its potential as a fusion energy source and the major role of DP in addressing key scientific and plasma issues. The DOE asked FEAC to review the IFE program in the context of the restructured fusion energy sciences mission. This review has been completed and will assist the OFES in planning the future direction of the IFE program.

Pursue Fusion Energy as an International Collaboration

Fusion energy offers the long-term potential to provide an environmentally attractive and economically viable energy option for a growing world population in the next century. As discussed in the PCAST and FEAC reports, energy availability will be a critical need for the economic growth of the less developed countries in the next century, and it could become a major determinant of global political stability. Because the development cost of fusion energy is high and the scientific challenge is great, fusion energy can most effectively be pursued through international collaboration.

Although the United States is unable to pursue an independent fusion energy development program, it is in the national interest to remain a credible partner in the international fusion program aimed at energy development. Because Europe and Japan have energy situations different from the United States, fusion energy research has a higher priority in those countries. Europe's fusion program is about 2 1/2 times the size of the U.S. fusion program and Japan's program is about 1 1/2 times the size of the U.S. program. Benefits for the United States from collaboration in the international pursuit of fusion energy include: ensuring energy availability for a growing world population, enabling U.S. scientific and technological leadership in selected areas, positioning U.S. industry as a participant in the provision of energy technology, demonstrating U.S. reliability as a partner, and potentially alleviating the environmental problems of fossil fuels. To be a strong partner in the long-term pursuit of fusion energy, the United States needs a vigorous domestic program in fusion science and technology.

International collaboration involving the United States, the European Union, Japan, and the Russian Federation is now focused on the scientific base, technology development, and engineering design necessary to construct a long-pulse burning plasma experiment, ITER. A primary goal of the ITER program is to demonstrate the scientific and technological feasibility of fusion by achieving controlled ignition and extended burn of the fusion fuel. The broad physics and engineering challenges that ITER addresses are largely generic to any next step toward the goal of fusion energy.

The ITER program is now engaged in the conduct of the Engineering Design Activities (EDA) to produce a comprehensive engineering design of ITER and all technical data necessary for future decisions on the construction of ITER. The scientific research conducted in support of the ITER program is valuable and is consistent with the fusion energy science mission of the U.S. program. The United States will seek to meet its commitment to the successful completion of the ITER EDA in July 1998. Doing so will leave open the possibility of U.S. participation in ITER construction and other international collaborations to advance fusion energy science and technology. Such collaborations would be a modest cost, but high-leveraged investment for the United States.

The restructured U.S. program will make every effort to remain a credible partner in the international fusion program that includes both ITER and many smaller activities in all areas of fusion science and technology. Given the high projected cost of a burning physics experiment and the fact that the United States now funds only about one-sixth of the world effort, a strategy based on international collaboration on fusion energy research and development can be highly cost-effective.

Resource Requirements

The FY 1996 budget of $244 million (a 32% reduction compared to FY 1995) required difficult choices among: meeting U.S. international commitments to the ITER EDA; conducting scientific programs at major world-class research facilities located within the United States (facilities that are in a period of unprecedented scientific productivity); and terminating valuable elements of the core U.S. scientific program. The reduced budget has resulted in:

- a restructured, reduced U.S. contribution to the ITER EDA (the reduction was done in such a way as to minimize the effects on the project plan and on the other Parties);
- the foregoing of any significant new U.S. scientific research facilities (including termination of the Tokamak Physics Experiment just as it was ready to enter construction);
- severe cutbacks in the scientific programs conducted at the major U.S. tokamak research facilities;
- curtailment of university research programs in experimental plasma physics;
- termination of some critical enabling technologies programs; and
- a major loss of scientific and technical personnel from universities, national laboratories, and industry.

Implementation Actions (FY 1997 and Beyond)

The Department's FY 1997 budget submission has restructured the fusion program as recommended by FEAC. In response to congressional guidance, this Strategic Plan assumes that the restructured program will be implemented at a constant level of effort and will avoid future mortgages for large construction projects. At a constant funding level of about $255 million per year (about 4% below the level recommended by FEAC)[5], the following key actions will be initiated in FY 1997 and carried forward in later years to implement a restructured Fusion Energy Sciences Program:

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[5] About $9 million for program administration costs has been moved to another account.

Advance Plasma Science

- *Establish significant plasma science research efforts at U.S. universities, building over the next five years to a sustained level of about $10 million per year.*

This will initiate action to support the new policy goal of advancing plasma science in pursuit of national science and technology goals, as recommended by both the NRC and FEAC. Success in the form of significant and enduring contributions to the advancement of plasma science will require a sustained effort by the Fusion Energy Sciences Program, as well as close coordination with other agencies funding plasma science (e.g., the National Aeronautics and Space Administration and the National Science Foundation) and with the Department's Office of Basic Energy Sciences and DP.

Develop Fusion Science and Concept Innovation

- *Complete the most important remaining experiments on TFTR to allow it to be shut down during FY 1997 or FY 1998, after running for at least half of FY 1997.*

This will carry out the FEAC recommendation to retire TFTR "after a period of operation to extract the remaining scientific benefit from it," no later than 1998. Operation will focus on unique scientific experiments to be carried out to resolve several important physics research issues aimed at improved performance regimes and profile control in a deuterium-tritium plasma. Priorities for these experiments will be established by FEAC through its Scientific Issues Subcommittee (SciCom). This Subcommittee will also review the scientific progress on TFTR in mid-FY 1997 and recommend whether to continue with the program for up to one more year or to stop operations and begin to prepare for decommissioning. As pointed out in the FEAC Report, premature termination of TFTR would forego unique scientific opportunities and "it is unclear when these lost scientific opportunities would return."

Resources made available from the shutdown of TFTR after the completion of these experiments will enable other fusion research programs to be pursued more effectively, as also recommended by FEAC. After TFTR shutdown, PPPL will continue to be a center of excellence in fusion and plasma science, with renewed emphasis on theory and modeling, diagnostics and instrumentation, and innovative fusion experimental science.

- *Exploit the scientific potential of DIII-D and Alcator C-MOD and the leading smaller research facilities more fully. Continue exploitation of the scientific potential of DIII-D and Alcator C-MOD at least through 2001, including some upgrades, as user facilities.*

This will enable the scientific research programs at these facilities to be carried out on a highly productive and cost-effective basis. In addition to the contributions expected for the study of tokamak improvement, it is anticipated that the scientific output and emphasis on innovation will broaden in the restructured program. The FEAC SciCom, through FEAC, will recommend priorities among the research efforts conducted at these facilities.

The keys to conventional tokamak concept improvement are plasma control and particle and power exhaust. Both DIII-D and Alcator C-MOD are modern tokamaks with flexible control of the plasma shape, profile, transport, and boundary conditions. The DIII-D program is particularly well-suited to tokamak concept improvement research with outstanding shaping flexibility, multiple external heating sources, and detailed diagnostics that permit careful scientific investigation of plasma characteristics. Alcator C-MOD is the only shaped, high magnetic field tokamak in the world. Its compact design allows the investigation of numerous science issues at a much reduced cost in comparison to more conventional tokamaks. At lower fields, Alcator C-MOD is also capable of concept improvement experiments at relatively long pulses.

- *Increase support for research on alternative concepts physics.*

This will begin the implementation of the FEAC call for increased attention to alternative approaches to fusion, as well as the FEAC support for new experiments "including one or two smaller but scientifically aggressive new facilities, at least one taking advantage of the PPPL infrastructure."

Funding is planned for completing the design and initiating fabrication of components for the National Spherical Tokamak Experiment (NSTX) at PPPL. This project will be used as a national facility for studying concept innovations at a moderate cost (a total of about $20 million over three years). Funding increases are anticipated for several ongoing small but world-class alternative concept facilities that at present are significantly underfunded. Other actions to enhance the alternative programs may include an innovations initiative and new university scale experiments. In addition, a just completed FEAC review of alternative concepts provides strategic guidance for developing an enhanced alternative concepts program.

- *Provide increased support for theory and modeling.*

This will start to implement the FEAC call for a robust theory and modeling program, which can be a very cost-effective approach to scientific research. The development and use of improved theoretical and computational tools will provide and verify scientific understanding of high temperature plasmas. Emphasis will be placed on use of the Numerical Tokamak Project to simulate plasma physics behavior and on the development of mathematical models and other advanced computer techniques.

- *Provide modest increases in the materials and in the technology budgets.*

Research on radiation resistant, low-activation materials is central to fusion's ultimate environmental and economic attractiveness and will continue. In early 1997, the European Union, Japan, the Russian Federation, and the United States will complete a joint conceptual design of a fusion materials testing facility. At that point, the United States will have to decide whether to participate, at a modest financial level, in follow-on design and construction activities. In addition, enabling technology research in support of current and next generation fusion experimental devices will continue in order to ensure optimal advancement of plasma science. However, development of hardware components for energy applications (e.g., demonstration of critical fusion nuclear processes) will be deferred.

Pursue Fusion Energy as an International Collaboration

- *Maintain the ITER EDA commitment constant in as-spent dollars at the revised lower level.*

This will enable the U.S. program to complete work on the high priority tasks assigned to it (e.g., fabrication of the Central Solenoid Model Magnet and completion of testing of a high-heat flux ITER divertor). The United States will thereby meet its commitment to the successful completion of the ITER EDA in July 1998, obtain important scientific and technical information, and preserve the option for further cooperation internationally.

- *In FY 1997, decide whether to enter into negotiations with the European Union, Japan, and the Russian Federation concerning the possibility of U.S. participation at a modest financial level in the construction and operation of ITER.*

At current funding levels, the United States can only pursue fusion energy science and technology as an international collaboration. As recommended by FEAC, a decision to enter into formal negotiations on ITER construction would follow a U.S. Government review of the results of the ITER EDA, as represented by the Detailed Design Report to be completed in December 1996, and be made in consultation with our international partners. Possible U.S. contributions to ITER construction in areas where the United States has expertise include system integration, engineering and component production, diagnostics and control systems, as well as physics design. Any increase over the current ITER EDA funding level would require overall budget growth for the U.S. program. Nevertheless, the United States should be able to play an important role with a modest financial commitment, with annual contributions comparable to the present EDA annual funding amount.

Program Governance

Changes are being made at DOE to reflect the new mission and the changes in the policy goals. The Office of Fusion Energy, which is responsible for fusion program management within the Office of Energy Research, is being reorganized and has been renamed as the Office of Fusion Energy Sciences to reflect the restructured program. The number of people in the Office is being reduced to be consistent with its new science mission. The structure of the fusion budget has also been changed to reflect the program changes.

Along with the structural changes, a new decision-making process involving more fusion community input, more peer review, and the assistance of the FEAC SciCom is being installed. The peer review process will be used as the primary mechanism for evaluating proposals, assessing progress and quality of work, and for initiating and terminating facilities, projects, research programs, and groups. The FEAC Report provided a number of specific recommendations for guiding and implementing the major programmatic changes inherent in the restructuring in a smooth and effective manner. In accordance with the FEAC Report, the following specific actions are being taken:

- Fusion Energy Sciences Advisory Committee (FESAC) - The FEAC has been renamed as the FESAC to reflect its new focus and will be reconfigured to broaden representation from the scientific community and stakeholders. The FESAC will advise the Department on policy, goals, priorities, budget, direction, and program balance, including fulfillment of the changes involved in the restructuring.

- Scientific Issues Subcommittee - This is a continuing FESAC subcommittee composed of experts from the fusion community and selected other fields of science and engineering. SciCom provided the critical scientific assessments that underpin the restructuring recommendations contained in the FEAC Report of January 27, 1996, and will continue to provide the best possible scientific information to FESAC to aid in priority setting.

- User Access Working Group - This working group, composed of facility managers and user representatives, will be established to work with the Office of Fusion Energy Sciences to develop mechanisms for encouraging and funding the highest quality proposals from the broad fusion community for experiments on the major facilities.

Conclusions

The new mission and policy goals of the restructured Fusion Energy Sciences Program contained in this Strategic Plan are responsive to congressional programmatic and funding guidance. In addition to revising the program's mission and goals, the Department has set priorities and made hard choices to implement the restructured program with substantially reduced funding. The new strategy represents a shift away from a schedule-driven energy development program to a program that emphasizes the scientific knowledge foundations of fusion. The restructured program will foster progress and innovation in fusion science and will advance basic plasma science.

A key element of the new strategy is the use of international cooperation for the pursuit of fusion energy science and technology. Under the new strategy, the United States will play an important supporting role in the international pursuit of fusion energy development and will be able to exert leadership in selected areas of expertise. This includes fulfilling the U.S. commitment to the ITER EDA and leaving open the possibility of U.S. participation in ITER construction, as a modest cost but high-leveraged investment.

A new decision-making process will ensure the close involvement of the fusion science community in priority setting, and follow-on reviews will assist in guiding the implementation of the major programmatic restructuring of the Fusion Energy Sciences Program.

To carry out the new strategy successfully and thereby realize the significant scientific benefits and future opportunities possible from the program's restructuring, the Fusion Energy Sciences Program requires annual funding in the range of the $255 million level included in the FY 1997 President's Budget Request.

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